PROCEEDINGS

OF

THE ROYAL SOCIETY.

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May 2, 1850.

The EARL OF ROSSE, President, in the Chair.

The following papers were read:—

1. "On the Meteorology of England during the years 1847, 1848 and 1849." By James Glaisher, Esq., F.R.S.

At the commencement of this paper the author states, that during the three years ending December 1849, meteorological observations on a uniform system have been taken at nearly forty different places, situated between the latitudes of $49\frac{1}{2}°$ and 55°, and between the longitudes of $1\frac{1}{2}°$ east and $5\frac{1}{2}°$ west of Greenwich, at elevations varying from 30 to 350 feet above the level of the sea.

Many of the instruments which have been used he has himself selected, and prior to their use has determined their index-errors; he has also visited the greater number of the stations, and examined their localities, the position of the instruments, &c.

The results from each station at the end of every quarter were forwarded to him; these he tested in every possible way; and those returns only which were found good enough to stand this examination were published at the end of every quarter in the Quarterly Reports of the Registrar-General. The object of this paper, the author states, to be not only the deduction of mean values from the combination of all these published results, but also the deduction from them of formulæ, for the purpose of testing the accuracy of the observations generally, and thus ascertaining the degree of confidence which may be placed in meteorological observations as now carried on, and if possible the deduction of the errors of the readings of those instruments which had not been directly compared with standards.

He then gives the mean numerical values for the years 1847, 1848 and 1849 in different parallels of latitude. By comparison of these he deduces general formulæ, and then compares the results as found from observation with those deduced from calculation, for every place of observation in the year 1849. Proceeding to the consideration of the difference between these two sets of numbers, he finds them to be mostly small; and hence concludes that the instru-

ments are generally good, and that the observations have been carefully made. In three instances, however, he finds that the readings of the barometer are almost one-tenth of an inch too high, and he purposes to use the differences thus found as index-errors in the reduction of future observations made by these instruments, till their actual index-errors are determined by direct comparison with a standard barometer. In five instances the mean temperature of the year from observations differed almost one degree from that found by calculation. At one of these places only have the instruments been compared with standards, and hence it is very probable that the difference found at this place may be due to local causes, of which this difference is a measure.

He finds also, that, notwithstanding the decrease of temperature with increase of latitude, the temperature of the dew-point, at all places at about the same elevation, and distant from the influence of the sea near the south coast, is almost alike, and hence that the actual amount of water distributed in the atmosphere is the same: this result was unexpected, and if confirmed by subsequent observations will be important.

The author observes that the tables fully explain the peculiarity of the weather in the counties of Cornwall and Devon, and near the sea; the periodical ranges of temperature in these localities being much less than in others, though they are found to enjoy only the mean annual temperatures due to their latitudes.

He remarks that the agreement between the observed and calculated values being found to be so close, the mean meteorological elements for the year 1849 for any place in England may be computed, with a close approximation to the truth.

To his paper are appended some sheets of curves exhibiting the simultaneous results from all the places, and these show that if any two or more places be taken in the same latitudes, the curves are nearly parallel; but, that if curves of places whose latitudes are different be compared, the one is found to be much bolder than the other. He considers that these sheets show in a very satisfactory manner that very considerable confidence may be placed in the results, and that a great advance has taken place within the last few years in the care and attention to meteorological investigation.

2. "On the Temperature of Man within the Tropics." By John Davy, M.D., F.R.S., &c.

In a former paper which was published in the Philosophical Transactions for 1845, the author gave the results of an inquiry on the temperature of man in England, as measured under the tongue by a thermometer made for the purpose, and using certain precautions necessary to ensure accuracy. An inquiry of the same kind and with the same instrument he has conducted in the West Indies, extending over a period of nearly three years and a half. This is the subject of his present communication. For the sake of comparison, he has followed in it nearly the same order as in the former. The results are given in a tabular form, divided into sections, and are followed by an appendix in which are recorded the daily obser-

vations in monthly sequence, accompanied by observations on the pulse, respiration and atmospheric temperature.

The following are the principal conclusions which seem to be warranted by the results:—

1. That the temperature of man within the tropics, on an average, is nearly 1° higher than in a temperate climate, such as that of England.

2. That it is constantly fluctuating, in health, within a range of from 1° to 2° in the twenty-four hours.

3. That the order of its fluctuation is not the same as in England, being lowest in the early morning, after the night's rest, and not as in England, at night, before going to rest.

4. That all exertion, whether of body or mind, except it be very gentle, varies the temperature; that gentle exercise, as carriage exercise or slow walking, has a depressing influence.

5. That between the temperature of the surface of the body and that of the deep-seated parts, there is little difference, not exceeding on the average 2° or 3°, and often less; with which there appears to be connected increased activity of the function of the skin and a diminished action of the kidneys.

6. That in a healthy state of the system, increase of temperature from exercise or any other exciting cause, is of short duration, rapidly subsiding on rest, and commonly followed by some depression, *i. e.* below its average degree.

7. That in sea-sickness, except when severe, the tendency is to equalization of temperature; but when severe, to increase of temperature, the marked effect of deranged health, with few if any exceptions.

8. That a sea voyage without sea-sickness, has also an equalizing influence on the temperature, not preventing however its increase with increase of atmospheric temperature, and its lowering with diminution of atmospheric temperature.

The author expresses belief that the results obtained admit of practical application in relation to health and disease: on this part of the subject, however, as unsuitable to the occasion, he does not dwell, reverting only to the circumstance pointed out in his former paper and now confirmed, that variation, not equability of the temperature of man within certain limits, however produced, is conducive to health, presenting therein an instance of happy adaptation as regards his mode of life and sphere of action.

The Society then adjourned to the 16th of May.

May 16, 1850.

The EARL OF ROSSE, President, in the Chair.

The following papers were read:—

1. "On the Geographical Distribution of the *Bulimi*, a group of terrestrial *Mollusca*; and on the modification of their calcifying functions according to the local physical conditions in which the

species occur." By Lovell Reeve, Esq., F.L.S. Communicated by Gideon Algernon Mantell, Esq., LL.D., F.R.S.

The preparation of a monograph of the terrestrial genus *Bulimus* having enabled the author to collect good authentic data concerning the localities and circumstances of their habitation, this memoir embodies his observations on their general distribution under types and provinces of types, and on the relation between the substance and colouring of the shell, and the differences of vegetation, temperature, and other physical conditions under which it is formed.

Bulimi being of less fugitive habits than most tribes of animals, are distributed over the equatorial, tropical, and warm temperate regions of the earth in assemblages of species limited in their range, and, so far as regards the shell, of very distinct typical character. The soft parts are much less variable, and being naturally sluggish, with few means of transport, little migration occurs even where there are no such natural boundaries as seas, deserts, or mountain chains. Owing to their arboreal habits the author considers the *Bulimi* fitter subjects for investigating the laws of geographical distribution than the *Helices* which live more in the earth, and are less influenced by the conditions with which they are surrounded. The few *Bulimi* of ground habits differ typically but little in countries very remote from each other. The localities of about five hundred species are known, and the majority are registered with their circumstances of habitation. Their area of geographical distribution lies between 40° south and 35° north in the New World, and between 42° south and 60° north in the Old World; that is, between the southern extremity of Chili and Texas in the former, and between Van Diemen's Land and Sweden in the latter; and there is no country within this area of which the *Bulimi* do not form part of the zoology. Regarding the shell with reference to its distinctions of form, composition, and system of colours, for the little variation in the living animal seems inadequate to the purpose, the *Bulimi* are distributed over this area in seven provinces of about forty typical assemblages of species, of which three-fifths inhabit the western hemisphere, and the remaining two-fifths, with a wider range and greater local variety of character in conformity with the more varied arrangement of the land, inhabit the eastern.

The author distinguishes the typical provinces of distribution as Venezuelan, Brazilian, Chilian, Bolivian, Caucasian, Malayan and African, and passes through the consideration of each in detail. The conditions most favourable to the calcifying functions of the *Bulimi* are an abundance of decaying vegetable matter, with an equable temperature of from 80° to 85°, in dark, close, humid woods, among shady thickets or in ravines. Near the sea-level in thin calcareous soil, and in sandy plains, where the vegetation is scanty and parched, or in grassy savannahs, the shell is light and often vividly coloured. In species which burrow in the earth, the shell is mostly colourless, and often of glassy tenuity.

The highest condition of the genus is in intertropical America,

and its northern limit ranges in both hemispheres with the parallels of equal temperature laid down by Humboldt. The calcified condition of the genus corresponds also with the curves northward in the isothermal lines along the west coast of South America and those bending southward on the east side; the *Bulimi* having a colder aspect in Chili from the cold precipitated by the great Antarctic current of cold water which flows nearly to the Gallapagos Islands, than those of the opposite Brazilian coast which are affected by the equatorial current. In Patagonia the genus is suddenly arrested in a tropical condition by the recent geological changes that have taken place in that now barren and riverless country. The memoir proceeds to show that in the distant islands of the Pacific the *Bulimi* are curiously represented by other genera of terrestrial mollusks; whilst the species of islands approximating to continents, such as Trinidad, partake of the character of those of the main land.

The European species belong to the Caucasian type, which has its centre in Asia Minor, where the shell is mostly colourless, owing to the dry, juiceless, thorny character of the vegetation which affords little nutriment, and the *Bulimi* live mostly under blocks of wood or stone. This type reaches nearly to the south-eastern corner of Asia, where it is suddenly met at Birmah and in the Malacca peninsula by the richly-coloured Malayan type, which is so abundantly and beautifully represented in the islands of the Eastern Archipelago. The distribution of the genus among these islands is remarkably local. The *Bulimi* of North Africa partake of the character of those of Europe, whilst those inhabiting south of the equator belong to a totally different type.

The precise localities and circumstances of habitation of the various genera of Mollusca have as yet been too imperfectly noted by travellers to aid much in determining the laws relating to geographical distribution. The present summary of collected facts may prove suggestive of more careful observation, and, in the hands of those who are acquainted with the geological and physical history of the earth's surface, lead to important and interesting results.

The paper is illustrated with a map constructed with tints of shade, colours, and isothermal lines.

2. "On the influence of Physical Agents on the development of the Tadpole of the Triton and the Frog." By John Higginbottom, Esq. Communicated by Thomas Bell, Esq., Sec. R.S.

Mr. Higginbottom's experiments were made in different positions and degrees of temperature, many of them in a rock cellar 30 feet deep, where no solar light ever entered, and where the temperature varied only in the course of the year from 48° to 55° Fahr.

The results of numerous experiments showed that the development of the tadpole was principally hastened or retarded by temperature and the supply of food, and that, contrary to the opinion of many other observers, the presence or absence of light did not appear to exercise any perceptible influence upon their development.

3. "On the Algebraic Expression of the Number of Partitions of which a given number is susceptible." By Sir John F. W. Herschel, Bart., K.H., F.R.S. &c.

The object of this paper is to exemplify and extend the mode of analysis explained by the author in two former communications to the Royal Society "On the Development of Exponential Functions," and "On Circulating Functions," to a case in the theory of numbers in which they afford remarkable facilities, viz. that of the partitions of which a given number is susceptible. The separation of the symbols of operation from those of quantity, in the mode explained in the former of those communications, allows of the expression of the sums of certain series entering into this theory, under a form susceptible of resolution (by a theorem here given) into two portions, one of which, a rational function of the independent variable or number to be subdivided, expresses approximately, as a rational fraction, the number of partitions; the other, a periodic or circulating function, expresses the fraction, less than unity, by which the other portion differs from an exact integer number, and which, applied with its proper sign to that former portion, reduces it to an integer. In the mode of procedure here followed all subdivision into cases according to the numerical constitution of the number to be subdivided is avoided, and a uniform treatment is carried throughout.

May 30, 1850.

The EARL OF ROSSE, President, in the Chair.

Sir Benjamin C. Brodie, Bart., gave notice, that, at the next ordinary Meeting of the Society, he would propose the Right Honourable Lord Londesborough for election into the Society.

Mr Weld communicated the following particulars respecting the original model of the safety-lamp which was presented to the Society at this meeting by Joseph Hodgson, Esq., F.R.S.

In November 1815, Sir Humphry Davy read a paper before the Royal Society 'On the Fire damp of Coal Mines, and on Methods of lighting the Mines so as to prevent its Explosion.' In this communication he described a safe light, "which became *extinguished* when introduced into very explosive mixtures of fire-damp:" but as this fell short of the philosopher's wishes, he instituted a fresh series of experiments, which resulted in his invention of the safety-lamp described in a paper read before the Society in January 1816. "The invention," he says, "consists in covering or surrounding the flame of a lamp or candle by a wire sieve;" and he adds, "when a lighted lamp or candle screwed into a ring soldered to a cylinder of wire gauze, having no apertures except those of the gauze, is introduced into the most explosive mixture of carburetted hydrogen and air, the cylinder becomes filled with a bright flame, and this flame continues to burn as long as the mixture is explosive." The model in the possession of the Royal Society answers in every respect to

this description, and to the representation of the lamp which accompanies the paper. It was made by Sir Humphry's own hands, and given by him to Dr. Lee, now Lord Bishop of Manchester, whose father was Assistant Secretary to the Royal Society at the time of Davy's Presidency. The excessive simplicity of the contrivance is most remarkable; but this is one of the greatest advantages which attended the invention. As the author remarks in the paper just quoted, "All that the miner requires to ensure security, are small wire cages to surround his candle or lamp, which may be made for a few pence, and of which various modifications may be adopted. And the application of this discovery will not only preserve him from the fire-damp, but enable him to apply it to use and to destroy it at the same time that it gives him a useful light." A month after the invention Sir Humphry informed the Society that his cylinder lamps had been used in two of the most dangerous mines near Newcastle with perfect success.

The following papers were then read:—

1. "On the Structure of the Dental Tissues of Rodentia." By John Tomes, Surgeon-Dentist to Middlesex Hospital. Communicated by William Bowman, Esq., F.R.S.

The author in this paper relates the results obtained from an extensive series of investigations on upwards of sixty of the more typical members of the order Rodentia. He finds, that not only are the teeth of animals of this order distinguished by strongly marked structural peculiarities (hitherto not recognised) from other mammalian teeth, but also that the teeth of the several larger groups are distinguished from each other by modifications in what may be called the rodential type of dental tissue. Mr. Tomes pointed out, in a paper published in Part 2 for 1849 of the Phil. Trans., that in the teeth of marsupial animals the dentinal tubes are continued into the enamel. In the present communication he shows that the structural peculiarities which characterise and are confined to the teeth of rodents are also mainly resident in the enamel. The earlier pages of the paper are devoted to a description of those structural conditions which are common to the teeth of the whole order. Amongst these, the author finds that the extremities of the dentinal tubes, which in the lower part of the tooth communicate with the pulp-cavity, become in the extruded portion sealed up by the development of a layer of non-tubular tissue which is formed at and near the apex of the pulp-cavity. This closure of the dentinal tubes is not however confined to the teeth of Rodentia, but occurs in all teeth in those parts exposed to wear. A similar condition is found to obtain in the osseous tissue which forms the antlers of the Deer-tribe. If a portion of an antler previous to its losing its periosteum be examined, ordinary Haversian systems are found: but if an antler which has been shed be examined, each of the larger Haversian systems will be found to be lined by a layer of transparent tissue destitute both of lacunæ and canaliculi. The author considers these conditions to indicate the existence of a general law, viz. that dense tubular tissues, when about to be exposed to wear or about to be

cast off, are previously deprived of their connection with nutrient fluids by the development of a layer of non-tubular tissue between them and the vascular surface, with which they were originally connected and from which they derived their nutriment.

In order to facilitate description, Mr. Tomes proposes to use several terms as indicative of the arrangement and number of the component tissues of teeth, which he defines and describes at length.

A minute description of the dental tissues of upwards of fifty species of rodents forms the larger part of the paper. They are taken in the order proposed by Mr. Waterhouse in his arrangement of Rodentia published in Johnston's Physical Atlas. The author has followed this arrangement, because he finds that it accords with the modifications of the dental tissues. The incisors are described as possessing the most strongly marked and constant characters.

The anterior surface of the incisors of many rodents has a deep brown colour, which has been attributed to the presence of a layer of coloured cementum. The author states that the enamel fibres can be traced through this coloured part to the surface of the tooth, and hence regards the colour as a stain resident in the terminal ends of the fibres, and derives the presence of cementum in this part of the tooth.

The great distinguishing structural feature in this order of quadrupeds consists in a lamelliform arrangement of the fibres of the enamel in the inner division of that tissue.

The author then enters into an elaborate and detailed account of the peculiarities of this structure, as exhibited in the *Sciuridæ*, in the different members of the family *Muridæ* in the Hystricine family, the *Leporidæ*, &c., and finally considers the following conclusions as established by these details:—

"That the teeth of some species of the order have specific structural characters by which they can be distinguished from any other known teeth. That in the teeth of all the Rodentia, excepting the family Leporidæ, a portion of the enamel has a lamelliform arrangement of its fibres; that the enamel lamellæ have a different and distinctive character in each of the larger groups, and that the variety of structure is constant throughout the members of the same group;" —"and that the variety in the structure of the dental tissues, with a few isolated exceptions, justifies and accords with the arrangement of the members of the order into the several divisions proposed by Mr. Waterhouse, and deduced by him from the relations of the several parts of the skull."

It is stated at the conclusion, that the author purposes in a future communication entering into the development and the special adaptation of the peculiarities of the dental tissues which it has been the business of this paper to lay before the Society.

2. "On the Meteorology of the Lake District of Cumberland and Westmoreland, with a continuation of the results of experiments on the fall of Rain at various heights, up to 3166 feet above the Sea-Level." By J. F. Miller, Esq., F.R.A.S. Communicated by Lieut.-Col. Sabine, R.A., For. Sec. R.S.

In this communication are given the results of the observations of the quantity of rain which has fallen during the year 1849 at twenty stations in the valleys, and six mountain stations, varying in altitude above the sea from 500 feet to 3166 feet. There is also given a table of the temperature at Seathwaite in Borrowdale during the year 1849.

With reference to the mountain gauges, the author observes that, on the whole, the results are similar to those of the three preceding years, but, as might be looked for in a dry year like the past, the quantities of rain deposited at the various stations are more nearly equal than usual. With respect to the temperature, he observes that the statement he made in his last communication, that "the inhabitants of the Lake District valleys enjoyed a milder and more equable climate than the residents in the open country, and particularly in the winter months," is confirmed by the thermometrical results of the present winter (1849–50).

3. "On the relation of the Air and Evaporation Temperatures to the Temperature of the Dew-Point, as determined by Mr. Glaisher's Hygrometrical Tables founded on the factors deduced from the Six-hourly observations made at the Royal Observatory, Greenwich." By J. F. Miller, Esq., F.R.A.S. Communicated by Lieut.-Col. Sabine, R.A., For. Sec. R.S.

After pointing out the importance of the hygrometer, both in a scientific and a practical point of view, the author goes into the question of the advantages and disadvantages attending the use of Daniell's hygrometer, and the relative merits of this instrument and the dry and wet-bulb thermometers. Although satisfied of the accuracy of Mr. Glaisher's Tables (founded on the Greenwich Observations), which show at once the relation of the temperature of evaporation to that of the dew-point, he was unwilling to abandon the use of Daniell's apparatus for that of the wet and dry-bulb thermometers, slight as is the trouble of observing them, without personal experience of the correctness of the tables from which the dew-point was to be deduced. He therefore instituted a series of perfectly comparable observations by the two methods, and in this communication gives the results obtained from them during a period of twenty months. From a comparison of the dew-points determined by the two methods, he concludes that the results show in a striking manner the extreme accuracy of Mr. Glaisher's Tables, and afford additional testimony to the value of the Greenwich Hygrometrical Observations, and the resulting formula on which those tables are founded.

The author then refers to the subject of evaporation, and gives the results of his own observations at Whitehaven during six years, viz. from 1843 to 1848 inclusive. From these he states that the mean annual amount of evaporation is 30·011 inches; and the mean quantity of rain for the same period being 45·255 inches, the depth of the water precipitated exceeds that taken up by evaporation, on the coast in latitude $54\frac{1}{2}°$, by 15·244 inches.

June 6, 1850.

The Annual General Meeting for the election of Fellows was held this day,—

The EARL OF ROSSE, President, in the Chair.

The Statutes relative to the election of Fellows having been read,—

William Spence, Esq. and James Yates, Esq. were, with the consent of the Society, appointed Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected:—

William Henry Barlow, Esq.	Charles Handfield Jones, M.B.
George Busk, Esq.	James P. Joule, Esq.
Thomas Blizard Curling, Esq.	John Fletcher Miller, Esq.
George Edward Day, M.D.	Major Henry Creswicke Rawlinson.
Warren De la Rue, Esq.	Edward Schunck, Esq.
William Fairbairn, Esq.	Daniel Sharpe, Esq.
Robert James Graves, M.D.	John Tomes, Esq.
Levett Landen Boscawen Ibbetson, Esq.	

The Society then adjourned.

June 13, 1850.

The EARL OF ROSSE, President, in the Chair.

Warren de la Rue, Esq.	Daniel Sharpe, Esq.
James P. Joule, Esq.	John Tomes, Esq.
Thomas Blizard Curling, Esq.	

were admitted into the Society.

The Right Hon. Lord Londesborough was balloted for, and elected a Fellow of the Society.

1. "On Dynamical Stability, and on the Oscillations of Floating Bodies." By the Rev. Henry Moseley, M.A., F.R.S., Corresponding Member of the Institute of France.

The position into which a body will *first roll* by the action of any force tending to incline it from its position of equilibrium is essentially different from that in which it will finally *rest*. It is nevertheless with reference to the latter only that the stability of floating bodies has hitherto been considered. The object of this paper is to discuss the question of stability with reference to the *former*; to compare the stabilities of different vessels as regards rolling; and to determine under what conditions of form and loading a vessel will, when subject to given disturbing causes, roll the least. The stability of a body understood in this sense the author calls its *dyna-*

mical stability. It is a second object of the paper to determine the conditions of *quick* and *slow* rolling.

The remarkable disparities observed between different vessels in respect to rolling in the recent experimental squadrons give great interest to this inquiry. It is moreover of much practical importance on account of the detriment to which ships are liable by reason of their wear and tear from quick rolling, and its interference with their qualities as ships of *war*.

When a ship heels over its centre of gravity is vertically displaced, and also the centre of gravity of the water it displaces (technically called its immersion); and the author, in the first place, shows that the difference of these vertical displacements, with reference to a given inclination, multiplied by the weight of the ship, is a measure of its dynamical stability; so that if there be any number of ships, and a common inclination (say 20°) be assumed for all, if this difference be calculated in respect to each ship, and multiplied by the weight of the ship, then that in respect to which this product is the greatest would be dynamically the most stable ship, or would heel the least, if all were subjected to the same force of the wind or the waves under the same circumstances. Stated *fully* and under its most general form, this theorem is as follows:—

"The *work** which must be done upon a ship to cause it to heel through a given angle, is equal to that necessary to raise it bodily through a vertical height equal to the difference of the vertical displacements (when thus heeling) of its centre of gravity and that of the water it displaces."

The Lords Commissioners of the Admiralty having directed that this theorem should be subjected to the test of experiment, experiments were undertaken for that object by Mr. Fincham, Master Shipwright of Her Majesty's Dockyard, Portsmouth, and by Mr. Rawson, the particulars of which are given in this paper.

It was necessary for this verification to do a *given* amount of work upon a floating body, causing it to incline through a given angle, and then to ascertain whether, as the theorem states, this amount of work was that necessary to raise the vessel bodily through a height equal to the difference of the vertical displacements of its centre of gravity, and that of its immersion whilst in the act of so inclining. For this purpose a model vessel was floated in a tank, and being fitted with a mast and long yard, a weight was attached to one extremity of this yard, and the vessel allowed to heel over under the influence of this weight. The extreme inclination to which it heeled was then accurately ascertained by an ingenious method devised by Mr. Rawson, and the vertical descent of the deflecting weight measured. The product of this descent by the deflecting weight gave the *work done upon the body to incline it from its position of equilibrium*, and by the theorem this should be equal to the weight of the vessel, multiplied by the difference of the two vertical displacements spoken of above. The forms of the vessels experimented on were so selected that the positions of the centres of gravity of their im-

* Measured in lbs. raised one foot.

mersions could in every position be readily determined, and this difference therefore ascertained. Thus the verification of the theorem became in every experiment practicable, and in all the accordance of the experiment with the theorem was remarkable. Did not the demonstration of it rest upon a mathematical basis, these experiments would indeed themselves be sufficient to establish it.

In its general application to the conditions of the stability of a vessel, the author places this theorem under an analytical form sufficiently simple to be applied in practice, and involving no other data than such as may be determined by methods familiar to naval architects and generally assumed in their calculations.

With reference to the conditions of *quick* and *slow* rolling, the discussion of which is the object of the second part of the paper, it is necessary in the first place to determine geometrically the position of the *axis* about which the vessel is, at any given period of its inclination, rolling. It is shown to be perpendicular to two lines, one of which is a vertical line passing through the centre of gravity of the plane of flotation in that position, and the other a horizontal line passing through the centre of gravity of the vessel and parallel to the plane in which any point of the body is rolling. The position of the axis of rolling being thus known, the determination of the *time* of rolling is comparatively easy.

The author gives formulæ for the times of rolling and pitching, which, like those for the angles of rolling and pitching, have been subjected to the test of experiments detailed in the paper, and have in like manner been confirmed.

The apparatus used for determining the times of oscillation of the models was contrived by Mr. Fincham. An arm was fixed in the direction of the length of the floating body so as to project from its extremity, and to the end of this arm a pencil was fixed vertically. The vessel being then prevented from displacing itself laterally whilst in the act of oscillating on an axis passing through its centre of gravity and of which the extremities were received between vertical guides, as the vessel oscillated a line was traced by the pencil upon a piece of paper adjusted upon a board curved of a suitable form, which was carried along by clock work with a uniform motion in the direction of the length of the vessel upon a carriage, that traversed a railroad resting upon the edges of the tank. A zigzag line was thus described on the paper, each turn in which corresponded to an oscillation, and the distance between two successive turns determined—from the known rate of the motion of the carriage—the time in which the oscillation was made.

The formulæ given for the amplitudes and the times of oscillation afford the means of determining these, from the *lines* of ships, *before they are constructed*; and the author suggests that a vessel being fixed upon whose properties in respect to rolling are known, it would be expedient to compare with them those of all others which are proposed to be constructed; it being a possible thing (by the aid of such formulæ) to determine whether these will roll under the like circumstances through greater angles, or quicker than the standard

vessel, and to alter their lines so as to satisfy the conditions of stability and slow rolling to any required extent.

2. "Observations on 287 Thunder-storms made at Highfield House, near Nottingham, during the last nine years." By Edward Lowe, Esq., F.R.A.S. Communicated by John Lee, Esq., LL.D., F.R.S. &c.

The thunder-storms referred to in this communication are recorded in a tabular form, arranged according to their dates. In this table are given the date; the hour of the commencement of the storm; the mean height of the barometer to tenths of an inch; whether it is rising, stationary, or falling; the direction of the wind before the storm, during its continuance, and after its cessation; the maximum temperature on the day of the storm and on the day after; the minimum temperature on the night before and on the night after; and general remarks on the storms. This table is followed by remarks on particular storms recorded in it. In conclusion the author gives the results of his observations with reference to the number of storms in each year; the number in each month, with the hours at which they mostly occur in particular months; the number that have occurred with a rising, stationary, or falling barometer; the number in respect to the direction of the wind and of the current in which the storms moved; the number of storms that have occurred at the various heights of the maximum, and also of the minimum thermometer; the number in which the peculiar breeze that suddenly springs up on the commencement of thunder-storms has been well marked; the change in the direction of some of these storms, and indications of rotatory motion; and finally, the different atmospheric phenomena which have accompanied these storms.

3. "On a Dorsal dermal Spine of the Hylæosaurus recently discovered in the Strata of Tilgate Forest." By Gideon Algernon Mantell, Esq., LL.D., F.R.S. &c.

In the first discovered specimen of the remains of the fossil reptile named Hylæosaurus by the author, there were associated with the recognizable parts of the skeleton a series of thin, long angular processes, six or seven of which extended in a line nearly parallel with the upper part of the vertebral column: these bones are from four to seventeen inches in length. There are also several imbedded in various parts of the same block of stone; and in another specimen of this reptile, consisting of a considerable portion of the distal part of the vertebral column, similar angular bones are associated with the spine. The true nature of these processes, from their great size and osseous character, was deemed very problematical: Dr. Mantell, in his original memoir in 1832, regarded them as dorsal dermal spines that had formed a serrated crest which extended along the back of the Hylæosaurus, in the same manner as the horny dermal fringe in many species of Iguana, Cyclura, &c. Professor Owen, in his reports on British fossil reptiles, expressed his dissent from this opinion, and considered it more probable that the bones in question were abdominal ribs.

In a memoir on the Iguanodon and Hylæosaurus (Phil. Trans. 1849), Dr. Mantell states that he had been able to obtain slices of one of these spines for microscopical examination, and that their internal structure was identical with that of the acknowledged dermal scutes of the same reptile. Still the true form of the articulating surface of the base of these spines was unknown, every specimen being imperfect in this respect. At length, after the lapse of eighteen years, Dr. Mantell obtained, through the liberality of Mr. Peter Fuller of Lewes, from the very quarry in which the original specimen of Hylæosaurus was found, the spine figured and described in this communication, in which the base is sufficiently entire to show that the mode of implantation in the skin was identical with that of the true dermal scutes; thus confirming the author's original interpretation of these remarkable appendages having constituted a serrated crest along the back of the Hylæosaurus. The specimens, and the microscopical sections, were exhibited to the Society.

4. "On the Variations of the Sulphates and Phosphates in the Urine in Disease." By Henry Bence Jones, M.D., F.R.S.

The object of the paper is to show whether the sulphates in the urine are increased or diminished in any class of diseases. The corresponding variations of the phosphates were determined. The particular conclusions may be thus stated:—

1. In three cases of acute chorea the most remarkable increase was observed in the amount of sulphates in the urine. In the same cases the quantity of urea was very much increased. The quantity of urine made in twenty-four hours was not excessively diminished, and the total amount of earthy and alkaline phosphates was below the average amount, sometimes remarkably less than in health.

2. In delirium tremens and in other delirium a remarkable increase in the amount of sulphates in the urine was frequently observed, and the total amount of phosphates was in the same cases occasionally remarkably diminished; and the resemblance to the state of chorea was still closer, inasmuch as occasionally a very great excess of urea was found in these cases also.

3. In acute inflammatory affections of the nervous structures, during the most febrile symptoms, an increase was observed in the amount of sulphates in the urine; and the total amount of earthy and alkaline phosphates in these diseases was increased in the same proportion as the sulphates were increased.

4. In some slight and chronic diseases of the nervous structures no increase in the amount of sulphates in the urine was observed, excepting when sulphate of magnesia had been taken.

5. In acute diseases, in which neither the nervous nor the muscular structures were chiefly affected, no increase in the sulphates or phosphates was observed, except after sulphate of magnesia.

6. In chronic diseases, in which neither the nervous nor the muscular structures were chiefly affected, no decided increase in the sulphates or phosphates in the urine was observed, except after sulphate of magnesia. One case of exostosis may be regarded as a doubtful exception to this statement.

The genera conclusions are—

That in acute chorea, in which the muscles are in excessive action, the sulphates and urea in the urine are greatly increased.

That in delirium tremens the same state of urine is frequently met with when the phosphates are not at all increased.

That in acute inflammation of the nervous structures sulphates and phosphates are both increased in the urine.

That in chronic diseases of the brain, and in other acute and chronic inflammations, no increase of the sulphates is observed except after sulphate of magnesia.

The result is that muscular action increases the sulphates in the urine without increasing the phosphates; and that inflammation of the brain increases the sulphates as well as phosphates in the urine.

5. "Second Appendix to a paper on the Variations of the Acidity of the Urine in Health." By Henry Bence Jones, M.D., F.R.S.

In a previous paper and appendix, the effect of different diets, of sulphuric and tartaric acids, of caustic potash and tartrate of potash on the acidity of the urine was traced. In this appendix tartrate and carbonate of ammonia are the substances whose influence is determined, the object being to examine the comparative effect of fixed and volatile alkalies.

The first day two drachms of tartrate of ammonia were taken in distilled water, the second day 288 grains were taken, and the third day 177 grains.

Comparative experiments were made when no tartrate of ammonia was taken; the result was that tartrate of ammonia caused no perceptible diminution of the acidity of the urine. The difference between tartrate of ammonia and tartrate of potash may be shortly stated thus: two drachms of tartrate of potash made the urine alkaline in thirty-five minutes after it was taken, whilst three drachms of tartrate of ammonia produced no perceptible effect on the acidity of the urine.

The sesquicarbonate of pharmacy was then tried. The first day 18 grains were taken dissolved in distilled water, the second day 40 grains, the third day 80 grains. Comparative experiments were made without the volatile alkali, and it was found that in these doses carbonate of ammonia did not diminish the acidity of the urine; on the contrary, the acidity was higher than usual, and it was increased for twenty-four hours after the volatile alkali was taken.

Further experiments were made with 80 grains of carbonate of ammonia on two different days: no diminution of the acidity of the urine was produced on either day. The first day the quantity of urine was much increased, and thus probably an increase in the acidity of the urine was not evident. The second day on which the carbonate of ammonia was taken the increase in the acidity of the urine was perceptible.

Thus the effect of volatile alkali on the acidity of the urine is totally distinct from the effect of fixed alkali; and the author considers, that by determining the variations of the nitrates in the urine, the cause of this difference will be discovered.

6. "On the Temperature of Steam, and its corresponding Pressure." By John Curr, Esq. Communicated by J. Scott Russell, Esq., F.R.S.

In this paper, which is a continuation of a former paper bearing the same title, the author states that the law given in that paper, in reference to steam when superincumbent on the water in the boiler, may be rendered applicable to the determination of its pressure when insulated therefrom, as in the case of the expansive engine, of which the cylinder being in part filled with steam of the same temperature as that in the boiler, the communication is suddenly cut off, and the stroke is completed by the pressure on the piston of the steam whilst expanding within the cylinder. He considers that the power of expansive engines has been greatly overrated, instancing those of the Great Britain, which were of the estimated power of 1200 horses, but which he states he can prove did not exceed in actual power that of 300 horses. This he attributes to the inapplicability of Mariotte's law without a particular limitation. Having premised that "it is assumed that, by nature's law in the generation of steam, of the temperature 100° and of a pressure of 15 lbs. on a square inch, the density of the Matter of Heat, is to that of atmospheric air of the same temperature and pressure exactly as 1 to 2," he then gives general laws, by means of which, he considers, the pressure of steam when cut off from its generating source may be correctly estimated.

"In conclusion," the author states, "it may be said that, this and the former paper, both professing to give laws relative to the pressure and temperature of steam, *independently of experiment*, the theories proposed must in general be accepted as absolute truths, in case of being verified by experiment, or taken as vain attempts to subvert laws already firmly or sufficiently established."

7. "An Experimental Inquiry into the strength of Wrought Iron Plates and riveted Joints as applied to Ship-building." By William Fairbairn, Esq., F.R.S.

The object of the author was to determine by direct experiment the strength and value of rolled iron plates and bars of different forms in reference to their application to ship-building, and the construction of other vessels exposed to severe strain.

The experiments described in this paper were conducted with great care, and the irons used were purposely selected from those districts where the largest quantities were manufactured. The relative strengths of each kind, as also the deductions, are given in the order in which the experiments were made.

The author found the tensile strength of plates, when torn asunder in the direction of the fibre, and when torn asunder across the fibre, as nearly as possible the same. This fact is derived from twenty distinct experiments, and he attributes this equality to the improved method of manufacture which of late years has been introduced. This new system is described as rendering the plate more uniform by crossing the layers in the process of 'piling,' thus forming an alternate series of laminæ, whose fibres cross each other in the body of the plate.

Having ascertained the comparative value of the different manufactures of plate-iron, the author, by another class of experiments, investigated the different methods in use of joining the plates together by rivets. This appears to be an important section of the inquiry, as the relative strength of each description of joint within reach of the experiments is clearly ascertained, and their relative values of strength determined in reference to each other, and to the plates themselves. These were found after a careful investigation to be nearly as the numbers 100, 70 and 56; that is to say, the plate being taken at 100, the double-riveted joint was 70, and the single-riveted joint 56.

The resisting powers of plates, and the different kind of joints by which they are united, having been proved, the paper goes on to investigate the value of the different kinds of timber when applied to a similar purpose, and by a careful comparison of the results, it is ascertained that the tensile powers of each, compared with iron, amber representing unity, are in the following ratio, viz.

Ash as	1	:	2·94
Teak as....	1	:	3·33
Fir (good) as	1	:	4·16
Beech as ...	1	:	4·34
Oak as	1	:	5·00

From the above it is inferred by the author, that iron being five times stronger than oak, a vessel built of that material is neither so strong, nor yet so secure, as the iron ship.

The next subject of inquiry is the transverse strength of angle-iron, T-iron and other sectional forms which enter into the construction of the beams, frames and ribs of ships. These combinations were likewise put to the test of experiment. In this part of the inquiry it was soon ascertained that bars of angle-iron used for that purpose are not of the best form, but exhibited great weakness when compared with wrought-iron beams of the I form, having flanges on both sides. From these results the author recommends that a combination of angle-iron should be used for these objects, as explained in the paper, and shown in the diagrams which accompany it.

In order to render the inquiry of practical value, experiments were made on the resisting powers of plates, by forcing a blunt instrument with hemispherical end of three inches diameter through the plates. These experiments were again repeated under similar circumstances upon timber, and a comparison is drawn from these data as to the resisting power of each. These experiments are interesting so far as they establish the superior strength of iron when exposed to severe strain, as frequently occurs in vessels taking the ground upon boulder-stones, or any uneven surface.

These results being obtained, the author closes the paper with a series of experiments made at Woolwich Dockyard on the strengths and elongation of iron-bars. In this department some curious results are obtained, such as the fact that an iron bar, when elongated to a considerable diminution of its sectional area, was not reduced but rather increased in strength; and after repeated experiments, it was found that wrought-iron bars, when elongated or wire-drawn, were

considerably improved in their powers of resistance to a transverse strain.

8. "On extraordinary Oscillations of the Sea; with an account of some Observations in Mount's Bay." By Richard Edmonds, Jun. Communicated by Sir Charles Lemon, Bart., F.R.S.

In this communication the author notices many remarkable oscillations of the sea which had been observed nearly a century ago in Mount's Bay and Plymouth Sound, and also elsewhere. He then particularly describes some which have occurred more recently at the former places. Of these the following are the principal:—

On the morning of the 31st of May, 1811, the sea was observed to rise and fall rapidly from 4 to 8 feet.

On the 5th of July, 1843, the author witnessed oscillations of the sea in Mount's Bay.

In the evening of the 30th of October, 1843, oscillations of the sea were observed in Mount's Bay and at Plymouth.

On the morning of the 5th of July, 1846, immediately after a terrific thunder-storm, oscillations of the sea were observed at Marazion. The author remarks that the great storm which passed over England on this day raged in the Atlantic during the night of the 4th of July.

On the morning of the 1st of August, 1846, the sea at Penzance pier was observed suddenly to rise between 1 and 2 feet, and as suddenly to rush back. It is remarked that London and its vicinity were visited on this day by a most destructive hail- and thunder-storm.

On the 23rd of May, 1847, there were extraordinary oscillations of the sea, and a slight motion of the ground was felt on the cliff between Newlyn and Mousehole.

After referring to the theories which have been advanced in explanation of these phenomena, the author observes, in conclusion, that, from what he has stated on the subject, and from the fact of earthquakes, as well as extraordinary oscillations of the sea, having so frequently occurred during thunder-storms, he sees no difficulty in the supposition, that all the oscillations to which he has referred may have resulted from submarine shocks of the earth, occasioned by electrical discharges between the earth and the atmosphere, or between oppositely electrified portions of the earth.

June 20, 1850.

THE EARL OF ROSSE, President, in the Chair.

The following papers were read:—

1. "Observations on the Nebulæ." By the Earl of Rosse, Pres. R.S., &c. &c.

The object of this paper is to lay before the Royal Society an account of the progress which has been made, up to the present time,

in the re-examination of Sir John Herschel's Catalogue of Nebulæ published in the Phil. Trans. for 1833.

Before describing any of the interesting objects the peculiar features of which the extraordinary powers of the telescope employed for their examination has brought to our knowledge, the author enters upon some details concerning the instrument itself. This telescope, which for aperture and the consequent power it possesses for the examination of faint details must for a considerable time, at least, remain unrivalled, has a clear aperture of 6 feet, with a focal length of 53 feet. It has hitherto been used as a Newtonian, but by the easy application of a little additional apparatus it may be conveniently worked as a Herschelian; and the author thinks it not improbable that, in the further examination of the objects of most promise with the full light of the speculum *undiminished by a second reflexion*, some additional features of interest will come out.

The tube reposes at its lower end upon a very massive universal joint of cast iron, resting upon a pier of stonework buried in the ground, and it is counterpoised so that it can be moved in polar distance with great facility. The extreme range of the tube in right ascension at the equator is one hour, but greater as the polar distance diminishes. By a little subsidiary apparatus the movement of the telescope can be rendered almost exactly equatorial; but up to the present time this apparatus has not been used, as, without it, the movement was found to be sufficiently equatorial for such measurements as have been required. The whole mounting was planned especially with a view of carrying on a regular system of sweeping; but as yet the discovery of new nebulæ has formed no part of the systematic work of the observatory, the known objects which require examination being so numerous that hitherto the observers have been fully occupied with them.

A clock movement was part of the original design, but as yet the telescope is not provided with one, and the want of it has not been very much felt.

Various micrometers have been tried, but, upon the whole, the common wire micrometer with thick lines has been found to succeed the best; for the faint details of the nebulæ are extinguished by any micrometrical contrivance which either diminishes the light of the telescope or renders the field less dark; and thick lines have been found to be visible without illumination in the darkest night.

The telescope has two specula, one about three and a half, and the other rather more than four tons weight. Each is provided with a system of levers to afford it an equable support. Upon this system it was placed before it was ground, and has rested upon it ever since. The systems of levers with the mode of applying them in the support of the speculum are described in the paper, and also the precautions taken to guard against strain and consequent flexure of the metal. Notwithstanding these precautions, undoubted evidences of flexure in the speculum have occasionally shown themselves. It has not, however, been found that flexure, even to the extent of materially disfiguring the image of a large star, interferes

much with the action of the speculum on the faint details of nebulæ, although it greatly lessens its power in bringing out minute points of light, and in showing resolvability where, under favourable circumstances, resolution had been previously effected.

It is stated that, in the spring of 1848, the heavier of the two specula, for nearly three months, performed admirably, very rarely exhibiting the slightest indication of flexure. It then remained inactive for some time before and after the solstice, and when observations with it were again commenced, it was found to be in a state of strain. On cautiously raising it a little by screws, for the purpose of readjusting the levers, it was found that the unequal strain of the screws had produced permanent flexure, so that the speculum did not again perform well until after it had been reground. Recently an alteration has been made in the mode of supporting the lighter of the two specula, which now rolls freely on eighty-one brass balls that support it nearly equably. After referring to other causes of unequal action, among which the varying state of the atmosphere is one of the most serious, the author remarks that the Society will not be surprised should it be in his power at a future time to communicate some additional particulars, even as to the nebulæ which have been most frequently observed.

The very beautiful sketches which illustrate the paper, are, it is remarked, on a very small scale, but are sufficient to convey a pretty accurate idea of the peculiarities of structure which have gradually become known. In many of the nebulæ they are very remarkable, and seem even to indicate the presence of dynamical laws we may perhaps fancy to be almost within our grasp.

On examining these sketches, it will at once be remarked, as stated by the author, that the spiral arrangement so strongly developed in H. 1622, 51 Messier, is traceable more or less distinctly in several of the sketches. More frequently indeed there is a nearer approach to a kind of irregular interrupted annular disposition of the luminous material, than to the regularity so striking in 51 Messier; but it can scarcely be doubted that these nebulæ are systems of a very similar nature, seen more or less perfectly, and variously placed with reference to the line of sight. The author adverts to the description of this nebula by Messier, Sir William Herschel and Sir John Herschel, and remarks, that taking the figure given by Sir John, and placing it as it would be seen with a Newtonian telescope, we shall at once recognise the bright convolutions of the spiral which were seen by him as a divided ring: thus with each increase of optical power the structure has become more complicated, and more unlike anything which we could picture to ourselves as the result of any form of dynamical law of which we find a counterpart in our system. After pointing out the importance of measurements and the difficulty of taking them satisfactorily, the author states, that of a few of the stars with which the nebula is pretty well studded, measurements with reference to the principal nucleus were taken by his assistant Mr. Stoney in the spring of 1849, and that these have been repeated this year during the months of April and May, and also some mea-

sures taken from the centre of the principal nucleus to the apparent boundary of the spiral coils in different angles of position. A hope is then expressed that, as several of these stars are no doubt within reach of the great instruments at Pulkova and at Cambridge, U.S., the distinguished astronomers who have charge of them will consider the subject worthy of their attention.

The spiral arrangement of 51 Messier was detected in the spring of 1845, and in the following spring an arrangement, also spiral, but of a different character, was detected in 99 Messier. The author considers that 3239 and 2370 of Herschel's 'Southern Catalogue' are very probably objects of a similar character; and as the same instrument does not appear to have revealed any trace of the form of 99 Messier, he does not doubt that they are much more conspicuous, and therefore entertains the hope that, whenever the southern hemisphere shall be re-examined with instruments of great power, these two remarkable nebulæ will yield some interesting result.

The author briefly refers to the other spiral nebulæ discovered up to the present time, which are more difficult to be seen, and to clusters in the exterior stars of which there appears to be a tendency to an arrangement in curved branches. He then passes to the regular cumular nebulæ, in which, although they are perceived at once to be objects of a very different character, there still seems to be something like a connecting link.

Among the nebulous stars two objects are stated to be well worthy of especial notice—No. 450 of Sir John Herschel's Catalogue, and *ι* Orionis. A representation of No. 450, as seen with the six-feet telescope, is given. It has been several times examined, but as yet not the slightest indication of resolvability has been seen. The annular form of this object was detected by Mr. Stoney when observing alone, but Lord Rosse has since had ample opportunities of satisfying himself that the object has been accurately represented. A representation of *ι* Orionis is likewise given. The remarkable feature in this object, the dark cavity not symmetrical with the star, was also discovered by Mr. Stoney when observing alone with the three-feet telescope. Lord Rosse has since seen it several times and sketched it. A small double star *n.f. ι* has similar openings, but are not so easily seen. These openings appear to be of the same character as the opening within the bright stars of the trapezium of Orion, the stars being at the edges of the opening. Had the stars been situated altogether within the openings, the suspicion that the nebula had been absorbed by the stars would perhaps have suggested itself more strongly. As it is, the author thinks we can hardly fail to conclude that the nebula is in some way connected with these bright stars, in fact that they are equidistant, and therefore, if the inquiries concerning parallax should result in giving us the distances of these bright stars, we shall have the distance of this nebula.

The long elliptic or lenticular nebulæ are stated to be very numerous, and three sketches of remarkable objects of this class are given.

In proceeding with the re-examination of Sir John Herschel's

Catalogue, several groups of nebulæ have been discovered, in some of which nebulous connexion has been detected between individuals of the group, in others not. Sketches of some have been made and measures taken; but although the subject of grouped or knotted nebulæ is considered one of deep interest, it has not yet been proceeded with far enough to warrant entering upon it in the present paper.

The conclusion of the paper is occupied with remarks relating to each figure, in order to render the information conveyed by it more complete, and these are stated to be for the most part extracts selected from the Journal of Observations.

2. "Electro-Physiological Researches.—Ninth Series." By Signor Carlo Matteucci. Communicated by W. R. Grove, Esq., F.R.S.

In the first portion of this paper the author refers to a work recently published by M. Du Bois Raymond "On the law of Muscular Current, and on the modification which that law undergoes by the effect of Contraction;" which work M. Matteucci states obliges him to transmit to the Royal Society certain researches the publication of which he would otherwise have wished to delay. He then refers to his previous researches published in the Philosophical Transactions for the years 1845 and 1847, and in the Annales de Chimie for October 1847. From the experiments detailed in those papers, and from certain points of doubt which he indicates, he considers himself authorized in concluding that the development of electricity by muscular contraction still remains to be demonstrated by experiment, and that the phenomenon of induced contraction is still that which leads most directly to this result.

He then gives a series of experiments illustrated by figures, and from them deduces the following conclusions:—

1st. The cause of induced contraction, according to all analogies, is the same as that which produces contraction of the galvanoscopic frog in several of the experiments given.

2ndly. The cause of these contractions is evidently an electrical phenomenon developed in the act of contraction, and which consists in a different state of electricity in the different points of the contracted limb.

3rdly. This electrical phenomenon, like the contraction which produces it, lasts only for an instant.

4thly. These electric states, developed by contraction, tend to produce electrical currents which circulate in opposite directions across a conducting arch interposed between the two limbs, which contract at the same time.

The author further states, that, whatever the theory of these phenomena may be, it is certain that they demonstrate the production of an electrical *disequilibrium* in the act of muscular contraction. Upon the question whether the cause of the species of discharge described is a phenomenon analogous to that of electrical fish, or a change in the natural conditions of the muscular current, the author, though leaning to the former alternative, forbears to express a posi-

tive opinion, as further experiments are wanting to furnish sufficient grounds for a decision.

3. "On the Condition of certain Elements at the moment of Chemical Change." By B. C. Brodie, Esq., F.R.S.

This paper contains an experimental inquiry, founded upon certain theoretical considerations as to the condition of bodies at the moment of chemical change, with the discussion of which the introduction is occupied.

The author considers that the peculiar combining properties of the elemental particles of which chemical substances are composed, are due to a chemical polarity of the acting masses, which takes place at the contact of the bodies, and have only a remote relation to the electro-chemical nature of the isolated element. In support of this view are cited the phænomena of double decomposition, and the properties of the so-called "nascent" elements, which could never be inferred from the nature of the element when once isolated and formed. Double decomposition the author considers to be the true type of all chemical action. In the case of the bodies called compound, this polarity is manifested by the division of the substance into two parts, which are universally considered to stand to one another in a certain positive and negative relation; and also by the synthesis, which corresponds to this division.

The object of the paper is to point out that an analogous polar relation exists, at the moment of chemical change, between the particles of which the elemental bodies themselves are composed, of which condition we have evidence both when the isolated element is chemically acted on by other bodies, and also in certain cases of the formation of the element from its compounds, in which we have a division and synthesis of the element corresponding (so far as this polar relation is considered) to the division and synthesis of a compound body. The evidence of these statements is, that when the isolated element is chemically acted upon, we may observe in it (as manifested by its combining properties) the same polar or nascent state as is developed in compound bodies; and also that we have certain remarkable cases of the synthesis of the element, to account for which we must assume the same combining relation between its particles as between the particles of which a compound substance is formed. These statements are supported by numerous instances.

The experimental inquiry relates to a remarkable case of the formation of oxygen, in which the author considers that the mutual attraction of the particles of that element determines the decomposition of the substances from which it is evolved. The experiment in question is the mutual decomposition which the peroxide of hydrogen and certain metallic oxides, first discovered by Thénard, undergo when in contact. Thus the author regards, in this case, the decomposition of the metallic oxide as a phænomenon which may be represented thus:—

$$HO_2 + mO_2 = \overset{+}{H}\overset{-}{O}\overset{+}{O}\overset{-}{O}\overset{+}{O}\overset{-}{m} = HO + O_2 + mO,$$

the metallic peroxide being reduced by the polar particle of oxygen, as in other cases it might be by hydrogen itself. The proof that such a chemical relation really exists between the particles of oxygen, would be found in the proportion in which the two substances were reduced. The paper contains an elaborate inquiry on this point in the case of the chloride and of the oxide of silver; the general result of which is, that these substances are capable of being reduced in various but definite proportions, according as the conditions of temperature and mass are varied. All the terms of this series of ratios have not been determined; but it is ascertained that the relative loss for the two substances proceeds by intermittent steps, and that the whole action is confined between the limits of the ratio of equality on the one hand, and the purely catalytic action (in which the metallic oxide would suffer no reduction) on the other; neither of which limits is ever absolutely reached.

The constant loss of oxygen from the decomposing bodies in equal equivalent proportions is found in the reaction of the peroxide of barium with iodine in the presence of water. In this experiment, the water in the presence of the iodine is reduced just as the peroxide in the other experiments; but here the loss is constant, and the change may be represented thus:—

$$\underbrace{\overset{-}{I}\overset{+}{H}\overset{-}{O}O\overset{+}{O}\overset{-}{B}\overset{+}{a}}=IBa+HO+O_2.$$

In this experiment no oxide of iodine whatever is formed, and the author considers that the formation of the oxygen itself is here the corresponding fact to the formation of the iodous acid, which takes place when iodine acts upon baryta.

4. "The Calling of the Sea." By Richard Edmonds, Jun. Communicated by W. J. Henwood, Esq., F.R.S.

In this communication the author states, that in the neighbourhood of Penzance there is often heard inland a murmuring or a roaring noise, locally termed "the calling of the sea," which on some occasions extends to the distance of eight or ten miles; whereas, at other times, although to a person on the shore the sea may be equally loud, and the state of the weather may appear equally favourable, no sound from the sea can be heard at the tenth part of that distance. From his observations during six years, he concludes, that when the calling of the sea proceeds from a direction different from the wind, or when it occurs during a calm, it is usually followed within six hours by a wind from the quarter from which it is heard.

5. "On the Structure of the Membrana Tympani in the Human Ear." By Joseph Toynbee, F.R.S. &c. &c.

In this paper the membrana tympani is described as consisting of the following layers, which are quite distinct from each other, both as regards their structure and functions:—

1. Epidermis.
2. The proper fibrous layer, composed of—
   a. The lamina of radiating fibres.
   b. The lamina of circular fibres.
3. Mucous membrane.

One of the principal objects of the paper is to describe the structure and functions of the fibrous laminæ. Since the time of Sir Everard Home, who pronounced the layer of radiating fibres to be muscular, anatomists have differed in their views of the nature of the fibrous element of the membrana tympani. The lamina of radiating fibres, the outer surface of which is covered by the epidermis, is described as continuous with the periosteum of the external meatus. With the exception of the uppermost fibres, which on account of their being somewhat flaccid have been considered as a separate tissue under the name of "membrana flaccida," the radiate layer is composed of fibres which extend from the circular cartilaginous ring to the malleus, and they interlace in their course. These fibres are from the 4000th to the 5000th part of an inch in breadth.

The lamina of circular fibres consists of circular fibres, which are firm and strong towards the circumference, but very attenuated towards the centre. These fibres are so attached and arranged as to form a layer of membrane, which in a quiescent state is saucer-shaped. The fibres composing the circular are smaller than those of the radiate lamina, being from the 6000th to the 10,000th part of an inch in breadth.

The facts that appear to be adverse to the idea of the fibres of either layer being muscular are—

1. The absence of distinct nuclei in the fibres.
2. Their great denseness and hardness.

It is next shown that the four laminæ forming the membrana tympani are continuous with other structures, of which they appear to be mere modifications, and that not one is proper to the organ.

The tensor tympani ligament, which had not been previously noticed by anatomists, is particularly described; it is attached externally to the malleus, close to the insertion of the tensor tympani muscle, and internally to the cochleariform process.

The latter part of the paper is occupied by observations on the functions of the fibrous laminæ and of the tensor ligament of the membrana tympani; and it is shown that by these two antagonistic forces, the one tending to draw the membrana tympani inwards, the other outwards, this organ is maintained in a state of moderate tension, and is always in a condition to receive ordinary sonorous undulations.

6. "Investigations into the Structure and Development of the Scales and Bones of Fishes." By W. C. Williamson, Esq. Communicated by W. B. Carpenter, M.D., F.R.S.

In this memoir the author first points out the discrepancies that exist between the opinions of M. Mandl and M. Agassiz respecting the structure and growth of cycloid and ctenoid scales; and after

referring to the peculiar views entertained by each of these ichthyologists, he enters upon the investigation of a number of examples, including the Carp, Perch, Gray Mullet, Pike, Salmon, and especially the large scale of an unknown fish from the Bay of Dulse on the western coast of Mexico. The results of this inquiry lead the author to conclusions which differ considerably from those of both M. Agassiz and M. Mandl. He points out the existence of *three* distinct vertically-disposed structures in each of these scales; a lower one, consisting of membranous laminæ; a middle calcareous one, having a very peculiar structure and growth; and an upper one, also calcareous, but very distinct from the last, and which is variously disposed in different scales. In all it contributes the peculiar cycloid and other markings which ornament their surfaces; whilst in ctenoid scales, isolated portions of it appear to form the characteristic teeth which project from their posterior margins.

The author then developes in succession the peculiar and beautiful structures seen in the scales of Dactylopterus, Balistes, Loricaria, and various forms belonging to the Ostraciant group. Whilst these examples exhibit singular diversities of structure, they appear to be all modified forms of one common type.

From this branch of the inquiry the author proceeds to examine the other calcareous tissues existing in fishes, commencing with the endo-skeletons of the Sharks and Rays (Plagiostomes). He shows that the entire osseous elements of these vertebrates are constructed of a peculiar form of bone, which he designates "chondriform," being wholly developed either within the soft tissues of true cartilage, or of a modified form of cartilage. Fossil remains, having the same structure, have been found in the lias at Lyme Regis and in the coal-measures near Manchester and Leeds. He then examines in succession a number of the bones of the Common Pike, and shows that whilst all the osseous elements of these skeletons are developed in connexion with a cartilaginous matrix, their tissues are of two kinds, which remain permanently distinct. One of these is of the same chondriform character as that seen in the Plagiostomes, being developed in the interior of the cartilage; the other, which is produced either at the outer surface of the cartilage or in cavities left by the absorption of the latter tissue, the author terms "membraniform," being formed within the lamellæ of a fibrous membrane, of which the primary origin is doubtful. The peculiar relations which these various structures bear to one another in the different stages of growth are explained. The same process of inquiry is applied to many of the bones of the Perch, Cod, Haddock, Sharp-nosed Eel, Salmon, &c., in each of which peculiarities exist. In the latter example especially the entire skeleton consists of a modified form of chondriform bone, of which it exhibits two kinds. One of these resembles that of the Pike and other examples, both in its aspect and in the situations in which it occurs. The second form is more peculiar; whilst the way in which its growth is accomplished closely resembles that of membraniform bones produced by the calcification of fibrous periosteal membranes, its internal structure

shows that it is of the chondriform type, being developed in a thick leathery periosteum of fibro-cartilage, and which appears to be continuous with the true cartilage wherever the two come into contact.

It is not possible to give any definite idea of the detailed observations contained in a memoir of which so large a portion is devoted to minute investigation. The inferences which the author deduces from his facts are given in connexion with each of the several topics discussed; and in the concluding portion of the memoir he points out the bearing which they have upon some general questions in physiology. A close resemblance is shown to exist between the processes of calcification, as carried on in the fibrous tissues of fish-scales, in cartilages and in fibro-cartilages, in all of which the phænomena closely correspond; and the author thinks that the bones and teeth of mammals, in which the process is far less obvious than in these ichthyal structures, may be calcified in a similar way.

The important bearing of the membraniform kosmine structures (which closely resemble the different varieties of dentine) upon the generally received hypothesis respecting the growth of teeth is also discussed; and the author thinks there are such sufficient reasons for doubting the correctness of that hypothesis, as to render a review of the evidence upon which it is based very desirable.

The peculiar modifications which the homologues of the Haversian canals of anthropotomists present amongst different groups of fishes are pointed out, as well as the very near affinity which exists between bone, dentine, ganoin, kosmine, enamel; and the probability of a closer relationship between cartilage, fibro-cartilage and fibrous periosteum, is also suggested as a subject deserving further investigation. The field opened out to the physiologist and the microscopist in the department of ichthyology is almost boundless, being comparatively unexplored, whilst it promises a rich harvest to those who labour in it.

7. "On the Impregnation of the Ovum in the Amphibia." By George Newport, F.R.S., F.L.S. &c.

The author states that this communication to the Royal Society is part of a series of investigations on development, on which he has been for some years engaged, and which was commenced in a paper on that of the Myriapoda, published in 1841, in the Philosophical Transactions. The plan followed in these investigations has been to combine observations on the natural history of the animals with others on the conditions which affect their development, as the best mode of arriving at correct conclusions. The history of the discovery of what can now be proved to be the direct agent of impregnation, the spermatozoon, is then traced; and it is shown, that although within the last few years an opinion has been gaining ground that the spermatozoon, and not the *liquor seminis*, as formerly supposed, is the means of impregnation, no acknowledged proof has hitherto been given of the correctness of this opinion, and no refutation afforded to the theory that the *liquor seminis* is the part of the seminal fluid immediately concerned. The question of

the agency of the spermatozoon has thus remained open; and it is to this question, with a view first to supply proof from direct experiments of the fact of the agency of this body, as well as to examine into the circumstances under which this agency is exerted, influenced or impeded, that the present communication is especially devoted.

The author then traces the changes in the ovum within the body of the Amphibia, from a short time before the disappearance of the germinal vesicle to the period when the ovum is expelled before impregnation. The structure of the germinal vesicle in the ovarian ovum is shown to be an involution of cells, as stated by Wagner and Barry; but the author differs entirely from the latter respecting the mode of disappearance of the vesicle, and also respecting the part played by its constituents in the production of the embryo. He believes the included cells are liberated by the diffluence of the membrane of the germinal vesicle in the interior of the yelk, not in the centre of the yelk, but much nearer to the upper or dark surface than to the white or inferior, and at the bottom of a short canal, the entrance to which is in the middle of the upper or black surface at a point already noticed by Prevost and Dumas, Rusconi and Böa; and he thinks that it is due to the diffluence of the envelope of the vesicle in this situation that the moment of disappearance has not yet been observed. The germinal vesicle in the Amphibia always disappears before the ovum leaves the ovary, and escapes into the cavity of the abdomen. The mode in which the ovum, after leaving the ovary, is believed to arrive at the entrance of the oviduct is then stated, and the structure of the entrance in the intermedial space, as shown by Swammerdam, described.

The author then traces the changes in the impregnated and in the unimpregnated ovum after spawning, from the first minute to the segmentation of the yelk in the former, and shows that the appearances in the two are almost identical during the first ten or twelve minutes, but that after that time the changes in the unimpregnated ovum cease, while further changes take place in the impregnated. The yelk at the end of from twelve to fifteen minutes invariably then rotates, so that the dark surface becomes uppermost; and it constantly afterwards returns to this position, however much or frequently this may be changed. In about three hours the yelk becomes separated on the upper surface from the vitelline membrane, and a space or chamber is formed between the two. The yelk then becomes depressed on the upper surface, but is slightly elongated to an obtuse oval form, in the horizontal direction; and in about half or three-quarters of an hour afterwards begins to divide in the margin of the central spot or orifice, from which point the division, as already known, passes outwardly and around the yelk until the mass is divided into two portions. These changes do not take place in the unimpregnated ovum, which merely becomes somewhat oval, but does not divide; so that segmentation may be regarded as a proof that the egg has been impregnated, a fact that was of great use as a test in the following experiments. The susceptibility of the ovum to become impregnated, and the circum-

stances which affect this, are then shown to depend on the degree of expansion of the envelopes, the imbibition of fluid, the temperature of the surrounding medium, and the degree of aëration. The envelopes expand and imbibe fluid most rapidly during the first half hour after the egg has been laid, and the susceptibility is diminished in the inverse ratio of the expansion and imbibition. It is greatest during the first three minutes, but is very feeble at the end of half an hour. These conditions are greatly modified by temperature, and in a much less degree by the aëration of the ovum. Experiments in proof of these facts are detailed, especially with reference to the number of eggs segmented and of embryo produced, and their earlier or later appearance in proportion to the higher or lower temperature of the medium. In March 1849 the author put to the test the agency of the spermatozoa in impregnation, by an experiment long since employed by Spallanzani, and more recently by Prevost and Dumas, namely, by carefully separating the spermatozoa of the Frog from the *liquor seminis* by filtration, and employing these, with the filter-paper on which they had been collected, in experiments on some sets of eggs, and the liquor in others; and the result was, that almost every ovum became impregnated in the former, but scarcely a single ovum in the latter. The production of a very few embryos in the sets to which the *liquor seminis* was added, he attributes to the fact that the whole of the spermatozoa had not been removed. These experiments he has repeated during the present spring with still more decided results; not a single ovum becoming segmented, nor a single embryo produced when the *liquor seminis* was completely freed of spermatozoa. The author states that these experiments had been completed, and he was engaged in preparing the paper for presentation to the Royal Society, before he was aware that the physiologists above named, Spallanzani first, and afterwards Prevost and Dumas, had obtained similar results by filtration of frog's semen, although the fact of their observations has been almost overlooked. To them therefore he resigns the credit of the results; but as his own investigations have been so completely independent of theirs, from which also they differ in some respects, he has felt it to be desirable still to give them in detail in this paper.

The direct agency of the spermatozoa in impregnation being thus proved, the author proceeds to investigate its nature. He first shows that the ova are not impregnated after the motive power in the spermatozoa has ceased. The period of duration of this power he finds to be much shorter than supposed by Spallanzani and by Prevost and Dumas; and he attributes the difference in length of time as observed by these authors and himself, to their having adopted the objectionable mode of procuring the seminal fluid by vivisection from the testes as well as the vesicular seminales, by which he conceives that spermatorial cells were obtained as well as mature spermatozoa, and that the former became matured only at a late period of the experiments. He differs also from Prevost and Dumas, and Dr. Martin Barry, with regard to the supposed pene-

tration of the spermatozoon bodily into the ovum. All the observations he has been able to make on the ovum of the Frog, both microscopically and experimentally, are opposed to the belief that any fissure or perforation exists in the envelopes of the ovum, as described by Dr. Barry in the ovum of the Rabbit, and through which the spermatozoon was supposed to enter. Neither is he able to confirm the statements of Prevost and Dumas, that the spermatozoa penetrate into the substance of the envelope of the egg either of the Frog or the Newt; and he thinks these distinguished observers must have supposed that spermatozoa which they saw on the exterior of the ovum were in the interior. The author has put this question to the test in the ovum of the Newt, *Lissotriton punctatus*. He extracted an ovum, which he had reason to believe had not been impregnated, from the oviduct, and placed it with seminal fluid in water, and immediately afterwards examined it with the microscope. Spermatozoa were detected upon it in less than one minute after immersion; but neither then, nor at any subsequent period, could even a single spermatozoon be seen within it, although the whole interior of the egg was brought within focus of the microscope, and distinctly recognised. This egg was preserved in a small glass capsule beneath the microscope, and watched until the embryo was produced. Spermatozoa were recognised on the exterior during the first forty-eight hours.

But although spermatozoa do not enter the interior, they are invariably found in contact with the surface of the impregnated ovum, and this contact is essential to their agency. The author also shows that the envelopes of the ovum are essential to its fecundation, and that ova taken from the ovary, or from the cavity of the body after they have left the ovary, but have not yet entered the oviduct and acquired their gelatinous coverings, are not susceptible of being impregnated. The coverings imbibe water by endosmose, but do not usually admit solid particles of matter equal in size to spermatozoa into their texture, as was proved by immersion in solution of carmine.

The author then enters at length on an examination of the agency of the spermatozoa as affected by chemical media. Availing himself of a fact ascertained during a chemical analysis of seminal fluid by Dr. Frerichs, that the spermatozoa are decomposed by caustic potash, he conceived the possibility of so employing this agent as to render it a test in experiment. Ova were passed from the body of a frog on a dry surface, without being in contact with water, until seminal fluid mixed with it was applied to them. After the lapse of a given time, solution of caustic potash, of sufficient strength to decompose the spermatozoa immediately, was also applied, and as quickly as possible afterwards was again diluted and removed with water, before the potash, as found by other experiments, acted prejudicially on the ova. The result was that segmentation of the yelk usually took place even when the interval of time between the application of the seminal fluid and the solution of potash was only one or two seconds, but no embryos were produced. When, however, the interval was five seconds, a very few embryos were formed;

but when the interval was fifteen or more seconds, they were produced in greater number. The conclusion deduced from these and similar experiments with nitrate of potash was, that impregnation is commenced almost at the instant of contact of the spermatozoon with the ovum; but that duration of contact, and possibly also diffluence of the spermatozoon and endosmosis of its substance, is necessary for fruitful impregnation. The experiments were varied by the application of the solution of potash before that of the seminal fluid, in which case the results were more unfavourable. With nitrate of potash, applied before as well as after the seminal fluid, the formation of embryos was not unfrequent. None however were produced when diluted acetic acid was used. This acid acts quickly and most unfavourably on the envelopes of the ovum.

The agency of the impregnating bodies was then tested in a similar way, by the application of solutions of gum-arabic and of starch, the action of which is merely mechanical. The results were similar to those with the potash.

When the gum or starch was applied, as in the case of the potash, *after* the application of seminal fluid in water, embryos were constantly produced, even when the interval between the two applications was only one second; but when either of these was applied to the ovum *before* the seminal fluid, then segmentation, if it occurred at all, took place very tardily. In general, however, no segmentation occurred, and no embryos, or but very few indeed, were produced.

These experiments, compared with those with potash, seemed to show that impregnation is commenced in a very short space of time, and that the spermatozoon is the agent immediately concerned; and that this agency is material in its operation, as seems to be shown in the fact that it can be prevented by the application both of chemical and of mechanical means to the ovum. We are thus led to infer, that although the spermatozoon does not bodily penetrate into the ovum, its first effect may have some relation to catalytic action, in inducing the segmentation of the yelk; and, having proof that fluids permeate the coverings of the ovum, we may hereafter find that the process is completed by the diffluence of the impregnating body, and the substance into which it is dissolved, by imbibition into the ovum by endosmosis.

One plate of the structures described accompanies the paper.

8. "A Mathematical Theory of Magnetism." By William Thomson, Esq., M.A., F.R.S.E., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow.

The Theory of Magnetism was first mathematically treated in a complete form by Poisson. Brief sketches of his theory, with some simplifications, have been given by Green and Murphy in their works on Electricity and Magnetism. In all these writings a hypothesis of two magnetic fluids has been adopted, and strictly adhered to throughout. No physical evidence can be adduced in support of such a hypothesis; but, on the contrary, recent discoveries, especially in electro-magnetism, render it excessively improbable. Hence it is

of importance that all reasoning with reference to magnetism should be conducted without assuming the existence of those hypothetical fluids.

The writer of the present paper endeavours to show that a complete mathematical theory of magnetism may be established upon the sole foundation of facts generally known, and Coulomb's special experimental researches.

The first part of the paper contains a general theory of magnets; the theory of magnetic induction, or of magnetization being reserved for communications which the author proposes to offer subsequently to the Royal Society. The five chapters which have already been communicated bear the following titles:—

In the second chapter the method of specifying, by "intensity and direction of magnetization" at every part of it, the magnetism of a magnet, is given; being founded on the elementary phenomena, and Coulomb's laws of magnetic force, which are explained in Chap. I. and the beginning of Chap. II.

In the third chapter, by a strictly synthetical investigation, corresponding closely with that investigation of "the equation of continuity" in fluid motion which is analogous to Fourier's investigation of the equation of motion of heat in a conducting solid, a certain distribution of "imaginary magnetic matter" consisting of equal quantities of positive and negative, or northern and southern matter each occupying finite portions of the body or of its surface separated from those occupied by the other, is shown to represent the polarity of a magnet according to the assumed properties of this magnetic matter. The formulæ by means of which the resultant action between two entire magnets of finite dimensions is determined are much simplified by this conventional method of representing polarity. The result of the investigation agrees with what is expressed by a certain formula of Poisson's, deduced by a process of *integration by parts*, from his elementary expression for the function since called by Green the "potential," at any point in the neighbourhood of a magnet. Hence the investigation of Chap. III. leads, as is shown at the commencement of Chap. IV., to a strictly synthetical proof of that remarkable formula.

The fourth chapter contains, in the first place, formulæ for the "potential," and for the "magnetic force," at any point in the neigh-

bourhood of a magnet, which agree with those of Poisson; and secondly, formulæ for the resultant action experienced by any finite portion of magnetized matter placed in a "field of force," either given, or determined by the preceding formula from a specification of the magnets to which it is due, by which the mathematical treatment, according to one method, of the problem which forms the subject of the chapter is completed. The chapter is concluded with the statement of a method of expressing the mutual action between two magnets by means of the differential coefficients of a function of their relative position, which is of importance chiefly because the principles on which it is founded lead to a new field of investigation in the theory of magnetism, having for subject the "mechanical value of magnetic distributions."

The fifth chapter contains, in the first place, explanations of the principal properties of the peculiar distributions to which the author has given the names of *solenoidal* and *lamellar*. A solenoidal distribution may be briefly defined as one of which the *polarity*, or the representative *imaginary magnetic matter* is entirely superficial; and a lamellar distribution, as one of which *the representative galvanism*, or the *resultant equivalent electrical currents*, are entirely superficial. If α, β, γ be the components of the intensity of magnetization at any point x, y, z of a magnet, the condition that the distribution of magnetism may be solenoidal is expressed by the equation

$$\frac{d\alpha}{dx}+\frac{d\beta}{dy}+\frac{d\gamma}{dz}=0;$$

and, again, the condition that the distribution may be lamellar is expressed by the three equations

$$\frac{d\beta}{dz}-\frac{d\gamma}{dy}=0,\ \frac{d\gamma}{dx}-\frac{d\alpha}{dz}=0,\ \frac{d\alpha}{dy}-\frac{d\beta}{dx}=0.$$

In the concluding part of Chap. V. three new methods of analysing the action of a magnet, suggested by the consideration of these special forms of distribution, and constituting, with Poisson's method mentioned above, a system of four expressions for the magnetic force connected with one another by certain analogies, are given. One of these (Poisson's) expresses the force at any point in terms of double integrals for the surface (the components of the force due to the superficial polarity); and triple integrals for the whole interior, involving $\frac{d\alpha}{dx}+\frac{d\beta}{dy}+\frac{d\gamma}{dz}$, and vanishing when this vanishes, *i. e.* when the distribution is solenoidal. The analogue of this expresses the force in terms of double integrals for the surface (the components of the force due to the superficial representative galvanism) and triple integrals for the whole interior, involving

$$\frac{d\beta}{dz}-\frac{d\gamma}{dy},\ \frac{d\gamma}{dx}-\frac{d\alpha}{dz},\ \frac{d\alpha}{dy}-\frac{d\beta}{dx},$$

and vanishing when these vanish, *i. e.* when the distribution is lamellar. Of the two remaining methods, one is confined to solenoi-

dal distributions, and expresses the magnetic force at any external point, or at any internal point in an infinitely small crevass tangential to the lines of magnetization, as the resultant of a certain distribution of tangential magnetization in an infinitely thin shell, coinciding with the surface, and occurred to the author as the analogue of a method which he had long before found for expressing the force due to a lamellar distribution. In this last-mentioned method, which is confined to lamellar distributions, the force at any external point, or at any interior point in an infinitely small crevass perpendicular to the lines of magnetization, is expressed as the resultant of a certain distribution of normal magnetization in an infinitely thin shell coinciding with the surface.

9. "Les Causes du Magnétisme terrestre prouvées.". Par M. Pierre Beron. Communicated by John Lee, Esq., LL.D., F.R.S. &c.

The author considers the elements of terrestrial magnetism to be, that the force with which the magnetic needle maintains its position is not everywhere the same, and that its declination and inclination vary from one region to another. These elements, he states, undergo very different modifications, which may be reduced to the following:—

1. Variations with reference to the position of the sun to the south or to the north of the equator; 2. diurnal variations in different regions of the earth; 3. disturbances which proceed from changes of weather, and from volcanic irruptions, and those which are observed during the appearance of the aurora borealis; 4. secular variations.

Adopting the views which have long since been put forward, but without adverting to the opinions of others who have preceded him in the same path, the author refers all the phænomena of terrestrial magnetism to the action of thermo-electric currents, and states, that as we know from climatology the regions of the earth which have the most hetero-thermal seasons, we have data for determining the intensity and direction of the thermo-electric currents in every region of the globe.

The hetero-thermal regions being marked by the isothermals of the most hetero-thermal months, the author distributes the thermo-electric currents into four magnetic systems, in each of which the currents are directed towards the middle, marked by the culmination of the isothermals. He then endeavours to trace out the general connexion between the thermo-electric currents which he assumes and the magnetic phænomena as observed in what he terms the American, the Asiatic, the Australian, and the Gallipagos' magnetic systems.

The extraordinary disturbances in the direction of the needle, he attributes to changes in the regular order of the distribution of solar heat, which give rise to corresponding changes in the thermo-electric currents; and the secular variation to the amelioration of climate arising from the culture of the soil in different regions of the earth.

The author concludes his memoir with the following remark:—

"Le magnétisme terrestre n'est donc que des courants thermo-électriques, et l'aiguille magnétique est un thermoscope qui nous indique les différences des températures des regions hétéro-thermes, dont nous connaissons les distances. Par suite le magnétisme terrestre fait une partie de la climatologie, qui exprime la cause de cette anomale distribution de la chaleur pendant chaque saison et pendant chaque mois. Cette cause se trouve,—1°, dans la conformation géographique de la surface de la terre; et 2°, dans les deplacements diurnes et annuels de notre planete."

10. "On the Physiology and Pathology of Phosphate and Oxalate of Lime, and their relation to the formation of Cells." By William Beneke, M.D. Communicated by Sir James Clark, Bart., F.R.S.

In this paper the author commences by referring to a work recently published by him, entitled "Der Phosphoräsure Kalk in physiologischer und therapeutischer Beziehung," Göttingen, 1850, in which he believes that he has established the indispensable necessity of phosphate of lime to the formation of cells in man, as well as in animals and plants; its deficiency as a cause of disease; and its efficacy administered internally as a means of alleviation or cure in the treatment of such disease. He cites from Liebig various proofs of the necessity of the presence of phosphate of lime for the formation of nitrogenous compounds in plants; and from Carl Schmidt, that it has an intimate relation to the formation of cells in invertebrate animals; and argues from his own experiments, that it has the same relation to the formation of cells in the higher classes of animals and in man. These experiments consisted, first, in the chemical examination of various pathological exudations, such as the serum produced by blisters, the secretions of wounds, ulcers, &c., the result of which satisfied him, that, wherever a formation of cells took place, phosphate of lime was present in considerable quantity; and wherever it was absent, he could not detect any phosphate. He believes that from a mixture of albumen, pure fat and phosphate of lime, put in a sand-bath at 104°, he has succeeded in artificially producing cells, which he describes and figures in various stages. He further adduces, in proof of his theory, the beneficial results of the treatment of various diseases connected with dyscrasia, by the administration of phosphate of lime. In such diseases he states that a much larger quantity of the phosphates is removed from the œconomy by the urine than in the normal state; and this he determines by a multitude of observations conducted on a method of analysis proposed by Dr. Heinz of Berlin. This increased elimination of the phosphates he attributes to the presence of oxalic acid, the existence of which in the urine he regards as always indicative of disease. On this subject he refers to the works of Dr. Prout, Dr. Golding Bird and Dr. Bence Jones, and compares the results of his own observations with those of the authors cited, giving figures of the various forms of oxalate of lime, and diagrams of the diurnal variations of the acidity of the urine, of its specific gravity, and of the phosphates and oxalates of lime contained in it, in two remarkable cases. From

a series of experiments on bones, he comes to the conclusion, that oxalic acid is the solvent for the phosphates in the animal œconomy; and deduces the production of oxalic acid, and especially of its hypernormal quantities,—first, from a hypernormal quantity of non-nitrogenous food, such as sugar, starch and farinaceous substances; and secondly, from want of sufficient oxygen taken from the air, as in malarious situations, or in the cases of persons suffering from disease of the lungs. The results of the author's observations are finally summed up in the following terms:—

1. That in the human œconomy, as well as in plants and in the inferior animals, the production of cells indispensably requires the presence of phosphate of lime.

2. That a deficient formation of cells in morbid affections of the system almost invariably indicates a deficiency of phosphate of lime; and that the administration of phosphate of lime has proved most beneficial in such affections.

3. That this deficiency of phosphate of lime is proved really to exist by the hypernormal quantities of phosphates eliminated by the urine in almost all the cases in which a deficient formation of cells or a want of flesh exists.

4. That this elimination of phosphates is caused by the oxalic acid which is produced in the œconomy in health as well as in disease, and causes the elimination as well of the normal as of the hypernormal quantities of the phosphates.

5. That the production of oxalic acid in preternatural quantities depends on different causes, the principal of which are,—the use of abundant quantities of saccharine and farinaceous food; the want of sufficient reception of oxygen by the lungs; a morbid decomposition of uric acid into urea and oxalic acid; and very probably the presence of abundant quantities of alkali in the blood.

6. That, consequently, by putting a stop to the production of hypernormal quantities of oxalic acid, we shall stop the elimination of hypernormal quantities of the phosphates, and consequently promote the formation of cells, supposing a sufficient quantity of nitrogenous and non-nitrogenous substances to be present.

11. "Supplementary Observations on the Diffusion of Liquids." By Thomas Graham, Esq., F.R.S., F.C.S.

The former experiments of the author furnished strong grounds for believing that isomorphous salts possess a similar diffusibility. All the salts of potash and ammonia, which were compared, appear to be equi-diffusive; so also were the salts of certain magnesian bases. A single preliminary observation on the nitrates of lead and baryta, however, opposed the general conclusion, and demanded further inquiry. It is scarcely necessary to say that any new means of recognizing the existence of the isomorphous relation between different substances, must prove highly valuable. The investigation was also extended to several new substances, such as hydrocyanic acid, acetic acid, sulphurous acid, alcohol, ammonia and salts of organic bases, without reference to isomorphous relations.

Hydrogen Acids.—The period of diffusion arbitrarily chosen for these acids was five days. The diffusate, or quantity of acid diffused, was determined by precipitating the liquid of the external reservoirs with nitrate of silver, and weighing the salt of silver formed.

Diffusion of hydrochloric acid in five days, at 51° F., in two cells:—

	Grs.	Ratio.
From 1 per cent. solution	7·41	0·97
From 2 per cent. solution	15·04	2·00
From 4 per cent. solution	30·72	4·08
From 8 per cent. solution	67·68	9·00

Hydrochloric acid is the most highly diffusive substance hitherto observed; it appears to exceed hydrate of potash at 53°·5, as 7·56 to 6·12, or as 100 to 80·9.

The experiments indicate a similarity of diffusion between the isomorphous substances, hydrochloric and hydriodic acids, and hydrochloric and hydrobromic acids.

Diffusate from 2 per cent. solutions at 51° F.:—

Hydrochloric acid	15·04	100
Hydriodic acid................	15·11	100·46

Diffusate from 2 per cent. solutions at 59°·7 F.:—

Hydrochloric acid	16·55	100
Hydrobromic acid	16·58	100·18

Hydrobromic acid appears therefore to coincide in diffusibility with hydrochloric acid at this temperature. It is remarked that these three acids, hydrochloric, hydrobromic and hydriodic, do not exhibit the same correspondence in another physical property, namely, the densities of their aqueous solutions containing the same proportion of acid. The densities of 2 per cent. solutions of hydrochloric and hydriodic acids appear to be respectively 1·0104 and 1·0143, at 60° F., and that of hydrobromic acid is an intermediate number. The same acids are also known to differ considerably in the boiling-points of solutions containing the same proportion of acid. A considerable diversity of physical properties appears here to be compatible with equal diffusibility in substances which are isomorphous.

The diffusion-time of bromine was made ten days, or double the time of hydrobromic acid. Two cells contained together a diffusate of 5·80 grs. of bromine; another two cells a diffusate of 5·88 grs.; mean 5·84 grs. at 60°·1 F.; or 6·76 grs. for a 1 per cent. solution. Doubling the last result, 13·52 grs. are obtained for a 2 per cent. solution, which is still considerably under the diffusate of hydrobromic acid (16·58 grs.) in half the time.

Hydrocyanic acid appeared less diffusive than hydrochloric acid, at the same temperature 59°·7, as 12·45 to 16·55, or as 75·2 to 100, and not to belong therefore to the same class of diffusive substances.

Nitric Acid.—Time of diffusion also five days. The quantity of

this acid diffused was determined with great exactness by neutralization by means of a normal solution of carbonate of soda.

The diffusion of the different proportions of this acid at one temperature is as follows.

Diffusion of nitrate of water in five days at 51°·2; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	6·99	0·95
From 2 per cent. solution	14·74	2
From 4 per cent. solution	28·76	3·90
From 8 per cent. solution	57·92	7·86

The 2 per cent. solution is taken as the standard of comparison for the ratios, instead of the 1 per cent. solution, from the greater accuracy with which the diffusion of the former can be observed.

The usual approach to equality of diffusion, between chlorides and nitrates, is observable in hydrochloric and nitric acids, at least in the 1 and 2 per cent. solutions.

Sulphuric Acid.—The time of diffusion arbitrarily chosen for this and the three following acids was ten days. The diffusate of this acid was determined in the same manner as that of nitric acid.

The diffusion of the different proportions of sulphuric acid is as follows.

Diffusion of sulphate of water in ten days at 49°·7; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	8·69	1·03
From 2 per cent. solution	16·91	2
From 4 per cent. solution	33·89	4·01
From 8 per cent. solution	68·96	8·16

The diffusibility of different strengths of this acid appears to be pretty uniform, but with a slight tendency to increase in the higher proportions, like hydrochloric acid.

Sulphuric acid is greatly inferior in velocity of diffusion to hydrochloric acid, but still appears to possess considerably more than half the diffusibility of the latter.

Chromic Acid.—The diffusate of the 2 per cent. solution was 22·43 grs. of chromic acid, in two cells, at 67°·3. The diffusion of sulphuric acid at 63°·5, was 19·73 grs., which would give about 21 grs. of that acid at 67°·3.

Acetic Acid.—This acid cannot be determined accurately by the acidimetrical method, owing to the acetates of potash and soda being essentially alkaline to test-paper, like the carbonates of the same bases, although neutral in composition. The weight of carbonate of baryta dissolved by the acid was had recourse to.

Diffusion of acetate of water in ten days at 48°·8; two cells:—

	Grs.	Ratio.
From 2 per cent. solution	11·31	2
From 4 per cent. solution	22·02	3·83
From 8 per cent. solution	41·80	7·26

The diffusibility diminishes with the larger proportions of acid.

This acid appears to be considerably less diffusive than sulphuric acid.

Sulphurous Acid.—Diffusion of sulphurous acid in ten days at 68°·1; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	8·09	0·954
From 2 per cent. solution	16·96	2
From 4 per cent. solution	33·00	3·891
From 8 per cent. solution	66·38	7·827

This substance appears to be less diffusive than sulphuric acid at the same temperature; the diffusion of sulphurous acid at 68°·1 considerably resembles that of sulphuric acid at 49°·7.

Ammonia.—Diffusion of ammonia in 4·04 days, the time of hydrate of potash, at 63°·4; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	4·93	1·029
From 2 per cent. solution	9·59	2
From 4 per cent. solution	19·72	4·117
From 8 per cent. solution	41·22	8·605

Ammonia appears to have a diffusibility approaching to that of hydrate of potash. It appears very similar to hydrocyanic acid at the same temperature; or to possess about three-fourths of the diffusibility of hydrochloric acid.

Alcohol.—Time of diffusion ten days. The quantity of alcohol diffused was determined by careful distillation. The density of the alcohol solutions in the phials was always made to exceed that of the water in the jars, by the addition of chloride of sodium to the former.

Diffusion of alcohol in ten days at 48°·7; two cells:—

From 2 per cent. solution	8·62
From 4 per cent. solution	16·12
From 8 per cent. solution	35·50

Alcohol does not appear to belong to the same class of diffusive substances as acetic acid, which might be expected from their similarity of composition, but possesses a considerably lower diffusibility.

Diffusion from 2 per cent. solutions in ten days:—

Acetate of water at 48°·8	11·51	100
Alcohol at 48°·7................	8·62	74·9

The diffusion of alcohol approaches to one-half of that of sulphate of water at nearly the same temperature.

Alcohol may be substituted for water to dissolve certain salts, and at the same time be made an atmosphere into which these salts are allowed to diffuse. From experiments which have been commenced on this subject, it appears that the diffusion of hydrate of potash, iodide of potassium, chloride of calcium and others is about four times slower into alcohol of density 0·840 than into water. The salts likewise often exhibit the same relations in their diffusibility in alco-

hol, as in water, with some singular exceptions, such as chloride of mercury.

Nitrate of Baryta.—The time of diffusion of this and the two following nitrates was 11·43 days*. The salt diffused was precipitated by sulphuric acid, and calculated from the weight of the sulphate of baryta formed.

Diffusion of nitrate of baryta in 11·43 days at 64°·1; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	7·72	1·026
From 2 per cent. solution	15·04	2
From 4 per cent. solution	29·60	3·936
From 8 per cent. solution	54·50	7·247

The diffusion of nitrate of strontia almost coincides with that of the isomorphous nitrate of baryta at the same temperature.

Diffusion from 1 per cent. solutions at 51°·5 in 11·43 days:—

Nitrate of baryta	6·73	100
Nitrate of strontia	6·79	100·89

Nitrate of Lime.—The diffusate was evaporated to dryness with an excess of sulphuric acid, and the nitrate of lime, which is always supposed anhydrous, was estimated from the sulphate of lime produced.

Diffusion of nitrate of lime in 11·43 days at 64°·1; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	7·66	1·021
From 2 per cent. solution	15·01	2
From 4 per cent. solution	29·04	3·872
From 8 per cent. solution	55·10	7·334

The results throughout for this salt are almost identical with those of nitrate of baryta, although these two salts differ greatly in solubility, and in one being a hydrated, and the other an anhydrous salt.

Acetates of Lead and Baryta.—Diffused for 16·166 days; the time chosen before for sulphate of magnesia, with seven days for chloride of sodium. These salts were diffused into water slightly acidulated with acetic acid.

Diffusion of 1 per cent. solutions in 16·166 days; two cells:—

Acetate of baryta at 53°·5........	7·50	100
Acetate of lead at 53°·1..........	7·84	104·53

Here, of two isomorphous salts, that of greatest atomic weight sensibly exceeds the other in diffusibility.

Chlorides of Barium and Strontium.—The diffusion of these salts being manifestly more rapid than that of the chloride of calcium, a shorter time was tried, which is to seven days, the time of chloride of sodium, as the square root of 3 to the square root of 4·5.

* This time is to that of sulphate of magnesia (16·166 days) as the square root of 8 is to the square root of 16; but does not appear to express the true relation between these salts.

Diffusion of chloride of barium in 8·57 days at 63°; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	6·32	1·047
From 2 per cent. solution	12·07	2
From 4 per cent. solution	23·96	3·970
From 8 per cent. solution	45·92	7·608

Diffusion of chloride of strontium in 8·57 days at 63°; 2 cells:—

	Grs.	Ratio.
From 1 per cent. solution	6·09	1·045
From 2 per cent. solution	11·66	2
From 4 per cent. solution	23·56	4·041
From 8 per cent. solution	44·46	7·626

Both the diffusates and ratios in the preceding table correspond closely with those of chloride of barium.

Chloride of Calcium.—The time for this and all the following magnesian chlorides and nitrates was 11·43 days.

Diffusion of chloride of calcium in 11·43 days at 63°·8; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	7·92	1·032
From 2 per cent. solution	15·35	2
From 4 per cent. solution	30·78	4·010
From 8 per cent. solution	61·56	8·021

On comparing solutions of chloride of calcium and nitrate of lime, it appears that the correspondence between the 1 and 2 per cent. solutions is sufficiently close, but that in the 4 and 8 per cent. the salts diverge, as happens also with hydrochloric and nitric acids themselves. The nitrate in both cases falls off, while the chloride sustains throughout the high diffusibility of the lower proportions.

Comparing certain other salts with chloride of calcium diffused at the same temperature, 50°·8, the following are the results:—

Chloride of calcium	6·51	100
Chloride of manganese	6·63	101·85
Nitrate of magnesia	6·49	99·69
Nitrate of copper	6·44	98·92

The following additional magnesian chlorides do not exhibit so close a correspondence as the preceding:—

Chloride of calcium	6·51	100
Chloride of zinc	6·29	96·61
Chloride of magnesium	6·17	94·77
Protosulphate of iron............	6·15	94·73
Chloride of copper..............	6·06	93·08

Sesquichloride of Iron.—A series of observations was made upon the diffusion of the different proportions of this salt from 1 to 8 per cent., but in all of them decomposition was determined by the diffusion, with turbidity also in the solution phial except in the 8 per cent. solution.

The mean diffusion from the 8 per cent. solution, at 63°·3, was

55·88 grs. of sesquichloride of iron, with 6·66 grs. of free hydrochloric acid, in two cells. It appears from this that perchloride of iron approaches the chloride of calcium in diffusibility. That the proto- and persalts of the magnesian metals should have a similar rate of diffusion, is not unlikely from other analogies which they exhibit.

Sulphates of Magnesia and Zinc.—The time chosen for the diffusion of these salts, namely, 16·166 days, is a multiple by 2 of the time of sulphate of potash, and by 4 of the time of hydrate of potash. The diffusate was evaporated to dryness and weighed.

Diffusion of sulphate of magnesia in 16·16 days at 65°·4; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	7·31	1·144
From 2 per cent. solution	12·79	2
From 4 per cent. solution	23·46	3·671
From 8 per cent. solution	42·82	6·701
From 8 per cent. solution at 62°·8..	42·66	1
From 16 per cent. solution at 62°·8..	75·06	1·759
From 24 per cent. solution at 62°·8..	102·04	2·340

Diffusion of sulphate of zinc in 16·16 days at 65°·4; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	6·67	1·091
From 2 per cent. solution	12·22	2
From 4 per cent. solution	23·12	3·784
From 8 per cent. solution	42·26	6·916
From 8 per cent. solution at 62°·8..	39·62	1
From 16 per cent. solution at 62°·8..	74·40	1·878
From 24 per cent. solution at 62°·8..	101·42	2·560

It is remarked that the diffusion of these two isomorphous salts, sulphate of magnesia and sulphate of zinc, differs so much, in the 1 per cent. solution, as 8·75 per cent. This, however, is considered to be an accidental error, the disturbances from changes of temperature and other causes of dispersion being in direct proportion to the duration of the experiment, and therefore much increased with long times; while the 1 per cent. solution also, from its low density, is the proportion most exposed to such errors. The sulphate of zinc appears to be the truest throughout, in its diffusion, of these two salts. The approach to equality becomes close in the 4 per cent. and larger proportions of salt, particularly with the unusually high proportions of 16 and 24 per cent., which were observed in these salts. The diffusion of both salts falls off remarkably in the higher proportions. The result of the comparison of these two magnesian sulphates is no doubt favourable to the similarity of diffusion of isomorphous salts.

Sulphate of Alumina.—The time of diffusion was the same as that for sulphate of magnesia.

Diffusion of sulphate of alumina in 16·166 days at 65°·4; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	5·48	1·074
From 2 per cent. solution	10·21	2
From 4 per cent. solution	19·28	3·780
From 8 per cent. solution	33·52	6·572

The diffusion of sulphate of alumina is very sensibly less than that of sulphate of zinc at the same temperature.

Nitrates of Silver and Soda and Chloride of Sodium.—Time of diffusion seven days.

Diffusion of nitrate of silver for seven days at 63°·4; two cells:—

	Grs.	Ratio.
From 2 per cent. solution	13·61	2
From 4 per cent. solution	26·34	3·87
From 8 per cent. solution	51·88	7·62

Diffusion of chloride of sodium and nitrate of soda in seven days, both at 63°·4; two cells:—

Chloride of sodium, 2 per cent. ..	12·37	100
Nitrate of soda, 2 per cent.......	12·35	99·83
Chloride of sodium, 4 per cent. ..	24·96	100
Nitrate of soda, 4 per cent.......	23·58	94·48
Chloride of sodium, 8 per cent. ..	48·44	100
Nitrate of soda, 8 per cent.......	47·74	98·55

As usual, the chloride is slightly more rapid in its diffusion than the nitrate.

Chlorides, Iodides and Bromides of Potassium and Sodium.—Time of diffusion 5·71 days. The salt diffused was treated with nitrate of silver, and the chloride of silver weighed.

Diffusion of chloride of potassium in 5·71 days at 62°; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	6·69	1·005
From 2 per cent. solution	13·32	2
From 4 per cent. solution	25·94	3·895
From 8 per cent. solution	53·64	8·054

The ratios are in remarkably close accordance with the proportions of salt diffused.

The times 5·71 and seven days chosen for the chloride of potassium and sodium, it will be observed, are as the square roots of 2 and 3. A certain deviation from this ratio of the times of equal diffusion, appears on comparing the experimental results obtained at present for these salts.

The following is the result of the diffusion of three isomorphous salts of potassium.

Diffusion of 2 per cent. solutions in 5·716 days, at 59°·8:—

	Grs.	Ratio.
Chloride of potassium	12·24	100
Bromide of potassium	12·46	101·80
Iodide of potassium.............	12·51	102·21
Mean............	12·40	

The corresponding salts of sodium were also compared.

Diffusion of 2 per cent. solutions for seven days:—

	Grs.	Ratio.
Chloride of sodium at 60°	12·14	100
Bromide of sodium at 59°·8	11·93	98·27
Iodide of sodium at 59°·8	12·18	100·33
Mean............	12·08	

In both groups there is a near approach to equality of diffusion. The times for the salts of the two bases being in the empirical proportion of the square roots of 2 and 3, the mean diffusates also approach pretty closely.

Bicarbonates of Potash, Ammonia and Soda.—Time of diffusion 8·083 days, or double that of hydrate of potash. The water of the jars was partially charged with carbonic acid gas, to prevent the decomposition of the bicarbonates in the act of diffusion.

Diffusion of bicarbonate of potash in 8·08 days at 68°·2; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	7·23	1·029
From 2 per cent. solution	14·05	2
From 4 per cent. solution	26·72	3·806
From 8 per cent. solution	52·01	7·408

Diffusion of bicarbonate of ammonia in 8·08 days at 68°·2; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	6·91	1·013
From 2 per cent. solution	13·65	2
From 4 per cent. solution	27·00	3·959
From 8 per cent. solution	50·10	7·346

The amount and progression of the diffusion of this salt correspond well, for all the proportions diffused, with the preceding isomorphous bicarbonate of potash.

Diffusion of bicarbonate of soda in 9·87 days at 68°·1; two cells:—

	Grs.	Ratio.
From 1 per cent. solution	7·31	1·059
From 2 per cent. solution	13·81	2
From 4 per cent. solution	26·70	3·869
From 8 per cent. solution	52·38	7·590

A remarkable approach to equality in the diffusion of the bicarbonates of potash and soda, in the times chosen, is observed equally in all the proportions of salt from 1 to 8 per cent.

The times chosen for the bicarbonates of potash and ammonia is to that of the bicarbonate of soda, as the square root of 2 to the square root of 3, and the remarkable agreement observed in the diffusion of these salts gives support therefore to that relation. In alluding to this relation, it is, however, added that the carbonates of potash and soda deviate from it in a sensible degree, and the hydrates of potash and soda very considerably; so that if the relation has a real foundation, it must be masked in the salts last named by

differences existing between them in certain properties, the discovery and investigation of which is of the last importance for the theory of liquid diffusion.

Hydrochlorates of Morphine and Strychnine.—Time of diffusion 11·43 days. The quantity diffused was determined from the chlorine, which was precipitated as chloride of silver in an acid solution.

These two analogous salts appear to approach very closely in diffusibility.

Diffusion from 2 per cent. solutions at 64°·1; two cells:—

Hydrochlorate of morphine......	11·60	100
Hydrochlorate of strychnine	11·49	99·05

The diffusion of these salts of organic bases in 11·43 days, is exceeded by the diffusion of chloride of ammonium or potassium in 5·71 days, or half the former time. The vegeto-alkalies appear thus to be divided from ammonia and potash.

12. "On the Mutual Relations of the Vital and Physical Forces." By William B. Carpenter, M.D., F.R.S. &c.

The purpose of the author in this communication is to show that the same kinds of "Correlation" as has been pointed out by Prof. Grove to exist among the physical forces,—light, heat, electricity, magnetism, chemical affinity, and mechanical motion,—exists also between these forces and those which operate in the development of living beings, and in the production of all their vital actions.

1. In some introductory remarks, the author briefly recapitulates the leading ideas which have been entertained by physiologists with respect to the relations between the physical and vital forces; and states it to be the doctrine at present in vogue, that vital forces are the manifestations of the dormant properties of organized structures, called into activity by the physical forces which operate as *stimuli*.

2. He then examines into the mutual relations of the several vital forces; and adduces facts and arguments to prove that the forces of assimilation, organization, chemico-vital transformation, and histological transformation, which are concerned in the development and maintenance of living organisms, are so many *modes* of one and the same vital force, whose most general operation is seen in *cell-formation*. And he then goes on to show that the production of the nervous and muscular forces, and of ciliary movement, are due to the same agency. Lastly, he points out that the nervous force, originating in one act of cell-formation, can in its turn influence other acts of the same kind, and can thus modify every other kind of vital operation. Whence he concludes that all these vital forces are "correlated."

3. The author then investigates the relations of the vital and physical forces to each other. Taking the nervous force as the most characteristic example of the former, he shows that it is correlated to electricity, heat, light, chemical affinity, and mechanical motion. He then endeavours to prove that the same correlation exists between *heat* and the organizing forces; so that the latter may be con-

sidered as in reality due to a transformation or conversion of heat by its passage through an organized structure; just as heat, acting through a certain mixture of metals, manifests itself as electricity. Hence he concludes that the physical forces are as closely correlated to the vital, as those of each category are to each other; the chief distinction between their respective operations being established by the speciality of the instruments through which they manifest themselves.

13. Letter from Lieut. Gillies, U.S.N., to Lieut.-Col. Sabine, R.A., For. Sec. R.S.

U.S.N. Astronomical Expedition,
Santiago de Chile, 20th Jan. 1850.

Dear Sir,—I had the pleasure to receive your letter of Aug. 3rd by the last monthly steamer from the north, and greatly lamented I could not answer it by the mail, which left Dec. 30th. Leaving the United States on the 16th of August, want of a proper amount of fire surface in the boilers of the steam-ship, caused my arrival at Chagres only on the day (27th) when the mail for Chile left. A month was thus unavoidably lost; for in anticipation of a passage through without delay, all instruments, except an aneroid barometer and thermometer, had been despatched round Cape Horn. With these such observations were made, until arrival here, as their construction permitted. From the indications of the aneroid there is a region extending from 200 miles to the S.S.W. of San Domingo to about 1° of north latitude on this side of the continent, where the pressure rarely exceeds 29·850 in., nor was the barometer but once in that whole distance as high as 29·900 in. At Panama the mean is 29·795 in. from observations at 9 A.M., 3 P.M., and 9 P.M., with a mean diurnal fall from the first to the second hours of ·08 in. The temperature for the same hours was 81°·0 with a range of 2°·9, and almost constantly saturated with moisture, though rain fell no more frequently than often occurs during the same period in the United States. As evidence of the hygrometric condition of the atmosphere, it was found impossible to dry clothing in my room after several days' open exposure, and they were finally exposed to the direct rays of the sun. Leather moulds in forty-eight hours. The light wind experienced was almost constantly from the northward and westward during the day, and variable at night. I think Lieut.-Col. Emery made observations for declination and dip *en route* for California, but nothing is known to me of the results, and I must await our return to give you data on these points. Should nothing intervene to change present intentions, I contemplate making observations at each of the fifteen ports where the steamer touches between Valparaiso and Panama. Nothing of note occurred during our passage to Chile. There was time to glance at Buonaventura, Guyaquil, Payta, Huanchaco (part of Truxillo), Casma,* five days at Lima, Pisco, Islay, Arica, Iquique, Coleiga, Copiapò and Coquimbo,—a multitude of little towns unimportant in themselves, and mentioned

only to indicate the points where I hope to obtain observations of the magnetic elements.

Reaching Santiago on the 27th of October, I was convinced in a brief time that no other part of Chile would so well answer the purposes of the expedition, and the Government here having acted promptly and with most commendable liberality on all points, there was no hesitation in selecting this city as my station. You know it is situated on a plain varying in width from twelve to forty miles, which, commencing just north of 33°, with a slight interruption in 34½°, extends to the Gulf of Onend in 41½°. The sea range of the Cordilleras, from which Santiago is distant from four to five leagues, has an elevation of 3000 to 4000 feet above the ocean, whilst the main chain to the eastward varies from 10,000 to 17,000 or 18,000 feet, and is distant about six leagues at the base. Interrupting the eastern horizon as they do, the interference with observations on the planet Venus in the morning twilight rendered so near an approach objectionable; but there was no locality in the vicinity of a proper residence free from this obstacle, and no place in the interior offering the facilities possessed by Santiago. If I mistake not, in one of my former letters I stated that the coast was impracticable, on account of very frequent fog and mist; and this was the opinion of the most observant residents here. There were two positions offered for our use by the Government,—a hill (Santa Lucia) in the eastern part of the city, with such rooms in the Castle, about halfway up, as might be needed, and the plain in the southern suburbs. The former has an elevation of some 200 feet, whilst the latter is half submerged during the rainy season, and almost inaccessible to pedestrians. Many reasons inclined me to prefer Santa Lucia, could its rocky crest be leveled, and this the Government at once undertook.

On the 9th of November, the ship having reached Valparaiso a few days after me, all the instruments *nearly*, together with the observatories, were deposited in the Castle, distant 100 miles from the ocean, having been conveyed in huge carts of the country drawn by five yokes of oxen. Chronometers, magnetical, meteorological, and all delicate instruments, were suspended by hide cords inside the carts, so as to swing free of each other, and everything came safely except three black bulb-thermometers, which Mr. Barrow had packed in sand. On opening the outer case (for the first time) the sand was found to have filtered through crevices in the packing-box, leaving the bulbs wholly unsupported. The fourth one was accidentally broken a few days after, and we are now with only one *spirit* thermometer for *radiation*, one of *them* having been, unfortunately, broken also. Our larger equatoreal was placed on its pier on the 6th of December, and on the 10th commenced the series of observations on Mars; since which time I have made more than 1100 differential measures. The superb meridian circle, made by Pistor and Martius, arrived here only at Christmas, and as it is an instrument of exceedingly limited adjustment, it has only been adjusted to the meridian a day or two. Instability of our floor rendering

additional support necessary for the joists when the extraordinary weight of the instrument is transferred to the receiving carriage, has rendered impracticable the adjustment of our microscopes thus far. But I hope to have the instrument fully at work by the 25th, stays having been driven in beneath the joists. A more exquisitely finished instrument was never turned out of a workshop.

Immediately after our location, meteorological observations were commenced, embracing eight periods of the day, at equal intervals, viz. at 3, 6, 9, 12 A.M. and P.M. Nothing is so striking to the stranger as the great dryness of the atmosphere and its almost perfect transparency, the ordinary difference of the dry and wet thermometers being above 12°, and, except over the Cordilleras to the N.E., clouds being rarely seen at this season of the year. Our barometer is of Mr. Hassler's form of construction, with a capacity above 0·6 in. Taking from the volume containing astronomical observations (no other being at hand), the meaus between 9 P.M. and meridian for the last twenty-one days of December, they are 28·126 in.; air 70°·3; wet 57°·8; and fluctuations during the same period respectively, 28·247 in. to 28·023 in.; 74°·4 to 65°·6; 61°·9 to 51°·3. From 10 A.M. till noon is the warmest period of the day; then a breeze sets in from S.W., which moderates the fierceness of the sun's rays until he sets. Our vicinity to the snow on the Cordilleras, N.E. from the city, renders night always pleasant, and indeed the temperature is never oppressive when not exposed to the direct action of the sun.

On unpacking the magnetical instruments, no top could be found for one set of the legs sent by Mr. Jones, and as it would be next to impossible to have a triangle made for securing them together, much time is unavoidably lost. One of the 3·67 in. magnets, too, is so greatly oxidized, I shall not use it until I hear from you again; and as there are no tidings of the altitude and azimuth instrument, the declinometer also must remain in its case. The hill on which the observatories have been erected being very decidedly polar, our observations for the horizontal force and dip have been made in a vineyard a few hundred yards to the eastward, and where we shall continue to make them for the present. Our own residence would have been greatly preferable; but the two portions being only about 16 feet square each, and having windows on at least two sides protected by stout bars of iron, it has been deemed objectionable. When the winter comes on it is probable a small abode house will be put up for them; at present we use them in the open air. The periods assigned for observation are, the 1st, 11th, and *term*-day of each month, commencing about 11 A.M. and ending about 4 P.M. This is all which my force will permit me to do; but if we can obtain means to mount the other tripod stand, so as to avoid loss of time in the vibration experiments, we shall have abundant leisure for the vertical and total forces as you suggest. Were the brass rings for determining the magnetic moments of the deflecting magnets measured and weighed by Mr. Jones? I have made out No. 12, outer diameter, 3·025 in.; inner, 2·338 in.; weight, 949 troy grains.

Since November 1st we have experienced five or six tremblings of the earth, principally during the first three weeks of that month. To my great mortification, the seismometer made no record either time, for this terrible visitant is of earnest interest to every one in Chile, and the object of the instrument, though greatly perverted, was a subject of much talk among the people of Santiago. The inverted pendulum has been made as sensitive as possible, and the recording pencil only pressed against the segment in the slightest manner to secure a mark—yet it has left no sign. True, the shocks were of no great violence, though strong enough to creak the ceilings overhead, and on one occasion to start every one into the street. Of the last I was warned by the rumbling noise, sufficiently long, to take out my watch and note the periods and intervals of its two shocks. To the best of our estimation the motion of the earthquake is invariably from the westward, and more frequently from W.S.W. than any other direction since November. One only of the phænomena, since our arrival, proved of serious injury; viz. that of November 18th, which kept the earth in motion in the vicinity of Coquimbo for the space of 84 seconds. Very slight injury was sustained by the houses from *this* cause; but it was immediately followed by a wave 16 feet above high-water mark, overwhelming the buildings and smelting furnaces on the shores of the bay. In Santiago I was roused from sound sleep ($6^h\ 10^m$ A.M.) by the rocking of my bed and creaking of the timbers overhead, to find all the inmates of the hotel flying to the streets. At Coquimbo shocks were repeatedly felt on the 16th, 18th, 19th and 20th, several each day. Dr. Lamont suggested an instrument to be formed of three upright cylinders wrapped with photographic paper and moved by clockwork; three beams suspended horizontally by two parallel threads each and a lamp in the centre. The subject was talked over with Prof. Henry prior to leaving the United States; and as I have since written him respecting the instrument here, it is greatly hoped one will be made after Dr. Lamont's suggestion and sent out to us.

I trust to hear from you as often as your leisure will permit, and you may feel assured your suggestions will always be gratefully received and carefully followed.

The Secretary of the Navy has authorized me to notify to all scientific correspondents, that their communications, if put under cover to "Honourable Secretary of the Navy, Washington," will be forwarded to me by the earliest monthly mail; and the Secretary of State has directed the "United States Consul, Panama," to receive and transmit free of expense any letters addressed to me. If, then, you will deposit your letters with Mr. Abbot Lawrence, they will assuredly come; or if put in the English West India mail, "care U.S. Consul, Panama," on the 1st, they will reach me on the 25th of the following month.

With my most respectful remembrances to Mrs. Sabine, believe me, dear Sir, Very faithfully yours,

S. W. Gillies.

P.S. 28th.—The most violent earthquake felt occurred on the night of the 20th, just before midnight. As usual, there were two shocks; noise with the first or preceding it, and the second most sensible; the former continuing four and the latter twelve seconds. Standing erect, the direction from which the noise and wave came was undoubtedly near W.S.W., and this position was assumed at the earliest symptom, that these facts might be more easily appreciated. Our seismometer gave no tidings.

Lieut.-Colonel Edward Sabine, R.A.,
F.R.S. &c. &c., Woolwich.

The Society then adjourned over the vacation to Thursday the 21st of November 1850.

November 21, 1850.

Dr. ROGET, V.P., in the Chair.

Dr. Graves was admitted into the Society.

The following gentlemen were elected Foreign Members:—

H. W. Dove.	J. E. Purkinje.
Joseph Liouville.	W. Weber.

November 28, 1851.

The EARL OF ROSSE, President, in the Chair.

William Fairbairn, Esq., Captain Ibbetson, and J. F. Miller, Esq., were admitted into the Society.

Dr. Faraday then delivered the Bakerian Lecture, which in substance was a résumé of the following papers:—

1. "Experimental Researches in Electricity." Twenty-fourth Series. On the possible relation of Gravity to Electricity. By Michael Faraday, Esq., D.C.L., F.R.S. &c.

Under the full persuasion that all the forces of nature are mutually dependent, and often, if not always, convertible more or less into each other, the author endeavoured to connect gravity and magnetic or electric action together by experimental results, and though the conclusions were, when cleared from all error, of a negative nature, he still thinks that the principle followed and the experiments themselves deserve to be recorded. Considering that some condition of the results produced by gravity ought to present itself, having a relation to the dual or antithetical character of the magnetic or electric forces, it seemed to the author that the approxi-

mation of two gravitating bodies towards each other, and their separation, were the only points which offered this kind of coincidence; and therefore, using the earth as one gravitating body, he employed a cylinder of metal, glass, resins, or other substances, as the other, and endeavoured to ascertain when the latter was allowed to fall, being surrounded by a helix of wire, whether any electric current was generated. Sometimes the cylinder was allowed to fall through the helix; at other times with the helix; and occasionally the helix was made the falling body. But when the various sources of error which sprung up were gradually removed, no traces of electric action remained which could be referred to the power of gravity.

In order to obtain a greater effect, an aparatus was employed (being nearly that used in the 23rd Series of these Researches) by which the effect of raising a body from the earth could be combined with that of a falling body by the fit use of commutators (if any action at all were produced). The apparatus was very good, and gave exceedingly delicate results, as was shown by other consequences of its action; but in respect of gravity it produced no effect whatever. Notwithstanding his failure in obtaining any experimental relation between gravity and magnetic or electric force, the author still expresses his conviction that there is a relation, and his hopes that it may be hereafter practically demonstrated.

2. "Experimental Researches in Electricity." Twenty-fifth Series. On the Magnetic and Diamagnetic Condition of Bodies. By Michael Faraday, Esq., D.C.L., F.R.S. &c. Received September 9, 1850.

As the author could find no polarity in diamagnetic bodies when under magnetic influence (a result described in the 23rd Series of these Researches), he endeavoured to discover some other physical condition of them, and of magnetic bodies, by which he might obtain an insight into their respective natures, and establish the true place of the magnetic zero; and considering the power with which a magnetic body moves, or tends to move, from weaker to stronger places of action, and that of a diamagnetic body to pass from stronger to weaker places of action, he hoped to obtain some results of condensation with the first class, and of expansion with the second, when they were subjected to very strong magnetic action; the respective bodies being selected from the class of gaseous substances, in which change of volume can be easily produced and measured. In the first place, therefore, a ray of light was passed over the surface of powerful magnetic poles surrounded by different gases, and the place of its source carefully examined by telescopes, micrometers, and other means, to ascertain whether the layer of air in contact with the poles was affected in its refracting force; but though the experiment was made in oxygen, nitrogen, and other gases, not the slightest effect was visible.

Resigning this process, therefore, two air-tight chambers were made, in which the magnetic poles formed the chief part of the internal surface of the chamber. The one was formed by bringing the flat ends of the two poles to within $\frac{1}{60}$th of an inch of each other, with a frame all round to form the sides; and the other by

cutting away the central parts of an iron cylinder so as to give it the form of an hour-glass, and then enclosing that part by an air-tight copper tube. Cocks were attached to these chambers for the introduction and removal of gases, and for the application of gauges, which were able to indicate a change of volume equal to the $\frac{1}{100000}$th part of the contents of the chamber. When any given gas was introduced into the chamber, and the latter then placed between the poles of the electro-magnet, any possible alteration of volume would be shown by the gauge as soon as the magnet was rendered active; but whatever gas was employed, or whatever power of magnet used, not the slightest change was produced.

Thinking it possible that there might be expansion in one direction and contraction in another, the gases were then examined as to the production of any currents in them, but no traces of such appeared.

From these results, the conclusion was arrived at by the author, that the motions of magnetic and diamagnetic bodies in each other do not appear to resemble those of attraction or repulsion of the ordinary kind, but to be of a differential action, dependent perhaps upon the manner in which the lines of magnetic force were affected in passing from one to the other during their course from pole to pole; the differential action being in ordinary cases between the body experimented with and the medium surrounding it and the poles. A method of showing this action with the gases is described, in which delicate soap-bubbles are made to contain a given gas, and these, when held in the magnetic field, approach, or are driven further off, according as they contain substances, magnetic or diamagnetic, in relation to air. Oxygen passes inwards or tends towards the magnetic axis, confirming the results formerly described by the author in his account of his investigations of flame and gases.

Perceiving that if two like bubbles were set on opposite sides of a magnetic core or keeper cut into the shape of an hour-glass, they would compensate each other, both for their own diamagnetic matter and for the air which they would displace; and that only the contents of the bulbs would be virtually in a differential relation to each other, the author passed from bubbles of soapy water to others of glass; and then constructed a differential torsion balance to which these could be attached, of the following nature:—A horizontal lever was suspended by cocoon silk, and at right angles, at the end of one arm, was attached a horizontal cross-bar, on which, at about $1\frac{1}{2}$ inch apart, and equidistant from the horizontal lever, were suspended the glass bubbles; and then the whole being adjusted so that one bubble should be on one side of the iron core and the other on the other side, any difference in their tendency to set inwards or outwards from the axial line caused them to take up their places of rest at different distances from the magnetic axis; and the power necessary to bring them to an equidistant position became a measure of their relative magnetic or diamagnetic force.

In the first place, different gases were tried against each other, and when oxygen was one of them it went inwards, driving every other

outwards. The other gases, when compared together, gave nearly equal results, and require a more delicate and finished balance to measure and determine the amount of their respective forces.

The author now conceived that he had attained to the long-sought power of examining gaseous bodies in relation to the effects of heat and the effects of expansion separately; and proceeded to an investigation of the latter point. For this purpose he prepared glass bubbles containing a full atmosphere, or half an atmosphere, or any other proportion of a given gas; having thus the power of diluting it without the addition of any other body. The effect was most striking. When nitrogen and oxygen bubbles were put into the balance, each at one atmosphere, the oxygen drove the nitrogen out powerfully. When the oxygen bubble was replaced by other bubbles containing less oxygen, the tendency inwards of the oxygen was less powerful; and when what may be called an oxygen vacuum (being a bulb filled with oxygen, exhausted, and then hermetically sealed) was put up, it simply balanced the nitrogen bubble. Oxygen at half an atmosphere was less magnetic than that at one atmosphere, but more magnetic than other oxygen at one-third of an atmosphere; and that at one-third surpassed the vacuum. In fact, the bubble with its contents was more magnetic in proportion to the oxygen it contained. On the other hand, nitrogen showed no difference of this kind; whether a bubble contained that gas more or less condensed, its power was the same. Other gases (excepting olefiant and cyanogen) seemed in this first rough apparatus to be in the same condition. The air-pump vacuums of all the gases were alike, including that of oxygen.

Hence the author decides upon the place for zero, and concludes that simple space presents that case. When matter is added to space it carries its own property with it there, adding either magnetic or diamagnetic force to the space so occupied in proportion to the quantity of matter employed; and now thinking that the point of zero is well determined, he concludes to use the word magnetic as a general term, and distinguishes the two classes of magnetic bodies into paramagnetic and diamagnetic substances.

There is no other gas like oxygen: its paramagnetic character is very high. A solution of protosulphate of iron in distilled water was prepared, of which a certain bulk in a glass bubble was of the same paramagnetic force as an equal volume of oxygen; the solution was then of such strength as to contain of crystallized protosulphate of iron seventeen times the weight of the oxygen which could counterbalance it. In another case, a glass bubble, containing one-third of a cubic inch of oxygen, was opposed to a corresponding bubble having within only an oxygen vacuum. As soon as the magnetic power was on, the oxygen passed inwards, and it required a force equal to one-tenth of a grain to hold it out at the equidistant position.

The author then refers generally to the air as a paramagnetic medium, because of the oxygen it contains, and in the next, or Twenty-sixth Series of Researches, he proposes to enter, after some preliminary inquiries, into the great subject of atmospheric magnetism.

3. "Experimental Researches in Electricity." Twenty-sixth Series. On Magnetic Conducting Power and Atmospheric Magnetism. By Michael Faraday, Esq., D.C.L., F.R.S. &c. Received October 9, 1850.

The remarkable results respecting oxygen and nitrogen described in the last Series, and the absence of any change of volume under strong magnetic action, led the author to apply for a time the idea of conducting power to the magnetic phenomena there described, meaning by that phrase the capability which bodies possess of affecting the transmission of the magnetic force without any reference to the process by which that transmission is effected; and assuming that two bodies are at the same time in the magnetic field, and that one displaces the other, he considers the result as a differential effect of their difference in conducting power.

If a free portion of space be considered with lines of equal magnetic force passing across it, they will be straight and parallel lines. If a sphere of paramagnetic matter be placed in such a space, they will gather upon and in the sphere, being no longer parallel in their course, nor of equal intensity in every part; or if a sphere of diamagnetic matter replace the former sphere, the lines of force will open out where the sphere is, being again no longer parallel in direction nor uniform in force. When the field of magnetic force is formed between the opposite flat ends of two large magnetic poles, then these are affected, and the globes also, and there are mutual actions; a paramagnetic body, if a little elongated, points axially and tends to go to the iron walls of the field, whilst a similar diamagnetic body points equatorially, and tends to go to the middle of the field. Paramagnetic bodies repel each other, and so also do diamagnetic bodies; but one of each class being taken, they attract one another.

The convergence of the lines of force upon the opposite sides of the paramagnetic sphere, and the corresponding divergence of them on the opposite sides of the diamagnetic sphere, the author expresses by the term *conduction polarity*. This polarity he carefully distinguishes from that which depends upon the reversion of the direction of the power; the latter he considers as a property of the particles of magnetic matter; the former as dependent rather upon the action of the mass: the latter is an absolute inversion of the direction of the power, the former only a divergence or deflection of it.

Applying the idea of conduction to magnecrystallic bodies, he concluded that the magnecrystallic axis would coincide with the direction of better conduction, and thence concluded, that, if a symmetric crystal of bismuth were carefully examined in different directions, it would be found to be less diamagnetic when its magnecrystallic axis was perpendicular to the axis of magnetic force of the field in which it was to be submitted to experiment, than when it was parallel to the magnetic axis. By means of the differential torsion balance described in the former paper, he was able to make the trial, and found the results were as anticipated. With calcareous spar and his present balance he was not able to establish any difference, but concludes that it will prove most diamagnetic when the

optic axis of the crystal and the magnetic axis of the field are parallel.

Advancing to the consideration of atmospheric magnetism, the author first refers to the earth as a source of magnetic power from which emanate lines of magnetic force passing into space according to a particular but recognized distribution, and in obedience to the general laws which govern the distribution of power about a given irregular magnet. In pure space the magnetic power is considered as transmitted onwards with a certain degree of facility which is constant, but may be increased or diminished by the presence of paramagnetic or diamagnetic matter within that space. The atmosphere is a portion of such matter, and can affect the magnetic lines which pass from the earth into space, and affects them differently according to variations which continually occur in it under natural circumstances. Four-fifths nearly by volume of the air is nitrogen, which is a gas that neither under any difference of temperature or of expansion shows any alteration in its power of affecting the transference of the magnetic force; whether added to space therefore in one state or another, or when undergoing changes of a corresponding kind by natural cause, it has no influence on the magnetic force. The perfect identity in magnetic action of hot and cold nitrogen, the author proves by new and delicate experiments. Oxygen forms the remaining fifth of the atmosphere. Its great magnetic changes by expansion have been described in the Twenty-fifth Series. Those produced by difference of temperature were described in the Philosophical Magazine for 1847, but are now resumed with more care, and found to belong to it alone, and not to nitrogen or to carbonic acid: as its temperature is raised its paramagnetic force diminishes, being resumed as the temperature falls again. These properties it carries into the atmosphere, so that the latter is in reality a magnetic medium ever varying, from the influence of natural circumstances, in its magnetic power. If a mass of the air be cooled it becomes more paramagnetic, if heated it becomes less paramagnetic (or diamagnetic), as compared with the air in a mean or normal condition.

The effect of the approach and retreat of the sun in his daily course is to produce such variations of changes in the temperature and expansion of the atmosphere as to influence the lines of force emanating from the earth, both in their direction and intensity; and the manner in which this influence will be developed is by means of figures and descriptions stated by the author in relation to the annual and daily variation, and the irregular perturbations of the magnetic force, which he thinks are consequences of it. He then applies the result of the magnetic observations at Hobarton as a test of the probable truth of the hypothesis, and considers that it affords strong confirmation. The upper or north end of the needle there goes west until about twenty-one o'clock, whilst the dip increases; the dip still increasing until noon, the upper end returns rapidly eastward, as the sun passes by, until two o'clock, the dip then decreasing; after which the needle goes west again, following the

sun. On examining the results at Toronto, corresponding effects were found to occur, when the upper or south end of the needle was considered, and therefore in accordance with the hypothesis. The examination of the observations made at Greenwich, Washington, Lake Athabasca, Fort Simpson, and St. Petersburgh, are considered as further adding confirmation. By the aid of these observations the author restates his principles more minutely, endeavouring to indicate what difference, changes in the inclination, declination, place of the sun, land, and sea, &c. will produce.

Though the sun is the cause of those changes in the atmosphere which affect the lines of force of the earth, he is not assumed as the centre of action as regards those lines; that is considered to exist somewhere in the atmosphere. It appears to be in the upper regions and not on the surface of the earth, because it increases the dip of places north and south of the tropics which have a certain amount of inclination, as at Hobarton and Toronto, both in summer and winter, but it diminishes the dip at places which are within the tropics, and with little inclination, as St. Helena. By other kinds of observations, it appears to be in advance of the sun. All the phenomena indicate that the sun does not act directly on the needles at different places, but mediately through its effect on the atmosphere.

The author then considers the possible cause of numerous irregular variations, such as those that are shown by the photographic processes of record at Greenwich and Toronto. The varying pressure of the atmosphere, the occurrence of winds and large currents of air, of rain and snow, of the passage of those masses of warm and cold air which the meteorologist recognizes in the atmosphere, of the aurora borealis, he considers may all produce changes in the lines of magnetic force, and become more or less sensible in the records of irregular variations. The author thinks it very possible that masses of air at different temperatures may be moved by the magnetic force of the earth, according to the principles of differential action made manifest in the experiments on warm and cold oxygen, in which case material as well as potential magnetic storms may exist. He concludes his paper by calling attention to the wonderful constitution of oxygen in its magnetical and electrical, as well as its chemical relations, to the offices it has to perform as part of the atmosphere.

4. "Experimental Researches in Electricity." Atmospheric Magnetism, continued. Twenty-seventh Series. By Michael Faraday, Esq., D.C.L., F.R.S. &c. Received November 19, 1850.

In order to obtain an experimental representative of the action of the atmosphere when heated above or cooled below the average temperature, the author employed a ring helix of covered copper wire, through which an electric current was passed. The helix was about one inch and a half in diameter, and having the well-known system of magnetic forces, was placed with its magnetic axis parallel to a free needle: when its position was such that a needle within the ring would point with the north end downward, then the effect in deflecting the surrounding lines of force of the earth was considered

as like that of a relatively paramagnetic mass of air; and when its position was reversed, its action was representative of that of a heated or relatively diamagnetic mass of air. Bringing this helix into the vicinity of small magnetic needles, suspended either freely, or so as to show declination or inclination, the planes of action or indifference as regards the power of deflecting the lines of force and the needle were observed. When the needle can move only in one plane, there are four quadrants, formed (in the case of the declination needle) by the intersection of the planes of the magnetic equator and meridian. When in these planes there is no deflection at the needle, but when in the quadrants there is, and in opposite directions in the neighbouring quadrants.

As the lines of force are held in and by the earth, so these experiments were repeated with a needle in near vicinity to a magnet, and the difference of effect is pointed out: then the extent to which these results are applicable to those of the earth is considered, and their utility in guiding the inquirer.

The effect of heated air having been considered in the last paper, that of cold air is now taken up; and after considering its action in causing a contraction or drawing together of the terrestrial lines of magnetic force, according to the principles of conduction before enunciated, the author considers generally where the regions of cold which travel round the earth every twenty-four hours will be in the northern and southern hemispheres, and how they will grow up and diminish in extent and importance as the sun moves north and south during the year. After which he applies these considerations, and the results of the experiments with the ring helix, to the explication of the changes of the needle as they are given by observations at St. Petersburgh, Greenwich, Hobarton, Toronto, Cape of Good Hope, St. Helena and Singapore. In doing this, he endeavours to explain the night action, the early morning effect, the contrary course of the needle for the same hours in different months, the difference of local time dependent on the distribution of land and water, the cumulative effect of preceding months, and the continual effect, especially in the tropical regions, of the higher temperature of the northern hemisphere above that of the south. In all these points the author sees such an agreement between the natural results and those which are suggested by the assumed physical cause of the magnetic variations, as to give him a growing confidence in the truth of the views he has put forth.

November 30, 1850.

At the Anniversary Meeting,

The EARL OF ROSSE, President, in the Chair.

Mr. Edward Solly, on the part of the Auditors of the Treasurer's Accounts, reported, that the total receipts during the past year, in-

cluding a balance of **£553 4*s*. 9*d*.**, amounted to **£3780 4*s*. 3*d*.**; and the total expenditure, during the same period, amounted to **£3573 5*s*. 7*d*.**, leaving a balance in the hands of the Treasurer of **£156 18*s*. 8*d*.**

The thanks of the Society were given to the Auditors for the trouble they had taken in examining the Treasurer's Accounts.

The thanks of the Society were given to the Treasurer.

List of Fellows of the Royal Society deceased since the last Anniversary (1849).

On the Home List.

Thomas Amyot, Esq.
Sir Felix Booth, Bart.
Sir Mark Isambard Brunel.
John Caldecott, Esq.
William James Frodsham, Esq.
Rev. William Kirby.
Right Hon. Sir Robert Peel, Bart.
Right Hon. Lord Petre.
William Prout, M.D.
Sir Martin Archer Shee.
James Smith, Esq.
Right Hon. Lord Stanley of Alderney.
Captain Owen Stanley, R.N.
James Thomson, Esq.
William Vaughan, Esq.

On the Foreign List.

H. M. D. de Blainville.
Joseph Louis Gay-Lussac.

Defaulters.

Charles Dickson Archibald, Esq.
John Burnet, Esq.
Right Hon. the Earl of Mountcashell,
John Scott Russell, Esq.

List of Fellows elected into the Royal Society since the last Anniversary (1849).

On the Home List.

William Henry Barlow, Esq.
George Busk, Esq.
Thomas Blizard Curling, Esq.
George Edward Day, M.D.
Warren De la Rue, Esq.
William Fairbairn, Esq.
Robert James Graves, M.D.
Levett Landen Boscawen Ibbetson, Esq.
Charles Handfield Jones, M.B.
James P. Joule, Esq.
Rt. Hon. Lord Londesborough.
John Fletcher Miller, Esq.
Major Henry Creswicke Rawlinson.
Edward Schunck, Esq.
Daniel Sharpe, Esq.
John Tomes, Esq.

On the Foreign List.

H. W. Dove.
Joseph Liouville.
J. E. Purkinje.
Wilhelm Weber.

The President then addressed the Meeting as follows:—

GENTLEMEN,

IT now becomes my duty, according to custom, to give a slight sketch of the events of the year which appear to be of most interest to us as a scientific body; but first permit me to express my deep sense of obligation to your Council. They have applied themselves so unremittingly to the discharge of the large amount of business which has devolved upon them, and have examined each question so thoroughly, that there has been little room for difference of opinion, and therefore my duties have been light, and free from anxiety.

Permit me also to return thanks to the distinguished men, Fellows of this Society, who, called in by the Council to aid them in wielding the new powers which they derive under the Government Grant, have rendered such important services.

You are no doubt aware that the first step taken by your Council in reference to the Government Grant, was to appoint a Committee, composed of the Council and an equal number of other Fellows, to make suitable regulations, and decide on the applications for aid in carrying out scientific objects. I am sure you will concur with me in thinking that your Council, in undertaking new and heavy responsibilities, where a great experiment was about to be tried, and where a false step at the beginning might have led to so much mischief, did wisely in seeking, in the varied talents and acquirements of a large Committee, a guarantee against any serious error.

I believe there were some not without their doubts as to the eventual success of the experiment, seeing that it had not always been an easy matter to apply the small fund previously available for similar objects: there were even some, I believe, who apprehended that abuses might spring up injurious to the Society. To me, I may perhaps be permitted to say, these doubts and apprehensions did not appear to be well-founded: it is but a short time since you conferred a Medal on M. Regnault for very able and elaborate researches, carried on at the expense of the French Government; if the French Government had not supplied the means, the experiments could not have been made. You are about to confer the Copley Medal on the distinguished astronomer, Professor Hansen, for his discoveries in Physical Astronomy; and it is a remarkable fact, that he has been for some time engaged in the construction of new lunar tables, the moon's place being so far in error as to produce serious practical inconveniences: this work was commenced in time of peace with funds provided by his own Government, but is now about to be completed with funds supplied to him by our Government. Here were two examples strikingly brought before us by our own recent proceedings, illustrating the position, that while the man of science supplies the mind, others often must supply the hands. There were many other similar examples; but, moreover, the *à priori* reasoning appeared to me to be conclusive; for if we except pure geometry, there seemed to be no other science where

there were not often constants to be determined at an expense of time and labour, which it would be a lavish waste to require from men of genius actually engaged in prosecuting original discovery. There was, I may add, in addition, the experience of the British Association and of foreign Governments. That there were men amongst us able and willing to enter the many tempting fields of scientific inquiry no one could doubt,—men ready to labour for us assiduously, provided that in doing so they were not called upon to make too great a sacrifice. As to abuses, it was evident that the Committee would frame regulations so as not only to make abuse impossible, but to remove all reasonable grounds even for the suspicion of abuse.

Soon after their appointment the Committee laid down certain rules and certain general principles for their guidance, which, although not irrevocably binding, nevertheless stand as precedents not lightly to be disregarded. The rule, that in every case there should be a Committee of three Fellows charged with the responsibility of seeing that the intentions of the Council in making the grant were strictly carried out, is in my opinion of great importance. Without such a rule it is probable that many of our ablest men, from a feeling of delicacy, coupled with an apprehension of a vague, indefinite responsibility, would hesitate to accept assistance. I will not detain you by entering into any further particulars, as all the proceedings are recorded on the Minutes of Council, to which you have access; but I am happy to be able to say, that although the season was far advanced before the intentions of the Council seem to have been generally understood, still there were many applications from men of eminence engaged in carrying out important objects, and even on this the first occasion there was no difficulty in making an effective appropriation of the grant.

Looking to the future, I think we may venture to anticipate that Parliament will annually place a similar sum at our disposal, so long as we can usefully employ it; and that while we shall thus be enabled to advance science in certain directions where otherwise we could have effected but little, we shall remain as free to exercise our own unbiassed judgements, and as perfectly independent as at any previous period of our history.

In this age of progress, when each month has its new facts, it would be vain to take a general survey of the discoveries of the year with a view of pointing out a certain number as the most important: experience has shown that time often affords the only real test of comparative value, as some discoveries apparently the most promising have in the end proved utterly barren; while others, at first sight of little moment, have led to the most brilliant results. It would be impossible, however, to look through the scientific publications of the year, however cursorily, without perceiving that much had been done. Our continental neighbours have been as energetic and as successful as usual; and I am sure you have been happy to observe, that in the United States, in the midst of the business and all-engrossing pursuits of a new country, science is engaging in-

creased attention; many very able men are springing up, and an association for the advancement of science has been formed and is well supported. Several observatories, furnished with the best instruments, principally from the manufactories of continental Europe, have been established. An astronomical expedition has been sent to the southern hemisphere, by authority of the federal government, under the command of Lieut. Gillies, principally for the purpose of making observations, which, used in conjunction with others in the northern hemisphere, may determine more accurately the parallax of the sun and the dimensions of the solar system. No observations have yet been received from the expedition, but it is known that an observatory has been established for its use at Santiago.

Much also has been done by the labour of private persons: an astronomical journal of great merit has been established; and several of the investigations published in it, as, for instance, on the velocity of the galvanic current, on the elements of Neptune, and on the wonderful comet of 1843, will probably be considered as standards.

One of these deserves especial mention. It is known that Bessel's examination of the places of Sirius and Procyon for many years past led him to the conclusion that these stars are subject to the action of some unknown force, and he suggested the attraction of invisible companions; now lately M. Schubert has examined, in the same way, the movements of Spica, and has come to the conclusion that it moves in an apparently small orbit, with a periodic time of forty years. Such conclusions, it is unnecessary to observe, are extremely doubtful, but the inquiries which have led to them are deserving of our best attention.

In the methods of observing, a step has also been taken by the astronomers of the United States which deserves particular notice. In a country in which the use of the electric telegraph is so extensively developed, it was natural that the application of this mode of instantaneous communication, to the determination of terrestrial longitudes, should soon suggest itself; indeed the principle had been suggested by Mr. Wheatstone as far back as 1840*; and it was then an easy step to make the galvanic wire an instrument for transmitting, not the comparison of the clock, but the actual observation of the transits over every individual wire of the transit telescope. For this purpose it was necessary in a series of stations to place only one moderately good clock, to be used in the following manner. An endless fillet of paper being carried by independent mechanism, under a style, which is pressed upon it by the action of a galvanic magnet, that derives its force from a wire passing through all the stations, and animated by a battery in any part; if the clock be so connected with the circuit that at every vibration of the pendulum the circuit is interrupted, the trace made by the style upon the paper fillet will be interrupted at every second. Now if at each of the observing stations the wire be carried through such an apparatus that the cir-

* Bulletin de l'Académie Royale de Bruxelles, 7th October 1840, and Comptes Rendus, tome xx. p. 1554.

cuit can at any instant be broken by a touch of the observer, it will be in his power, by a mere movement of his finger when the object passes each wire of the transit telescope, to exhibit in the impressed trace on the fillet a series of interruptions, peculiar to the observation, mingled in their proper places with the series of interruptions produced by the clock. As this can be done at each station without impeding the similar operations at the other stations, it is evident that several series of strictly comparable observations may be recorded on the same fillet of paper; one wire only being necessary, and indeed only one being applicable. It then became a question whether an analogous method of recording transits might not be available for the observations of a single observatory, without reference to the problem of determining terrestrial longitudes. For this purpose it appeared best to reverse the relations of the clock and galvanic current, so that the vibration of the pendulum should make an impress upon a fillet or revolving disc. Practically, it is found necessary to have another battery, another wire, another galvanic magnet, and another style for making the impression corresponding to each observed transit over a wire of the instrument: if it happens that there are in the same observatory several instruments where observations are to be comparable, by a proper arrangement of wires all these observations may be recorded by means of the same style. Numerical evidence is adduced to show that the irregularity of transits thus observed is far less than that of transits observed by the eye and ear; and there is no doubt that the number of observations made in a given time may be very much greater than that of observations made on the old system. On the other hand, the after-labour of reading off these graphical transits, and converting them into numbers, will be considerable. I am not aware that this method of observation has been used on our side of the Atlantic, although I am able to assert that it has engaged the attention of some of the English observers. Preparations are making at the present time for trying it at the Royal Observatory at Greenwich. It is proposed to record the transits taken with the new transit circle, and those taken with the altitude and azimuth instrument, on the same recording surface. Numerous experiments have been made in America by Mitchell, Walker and others, for determining with the aid of this system of record, the velocity of the galvanic pulse through the ordinary telegraph wires. The inquiry however is one of peculiar difficulty, and I am happy to find it is engaging the attention of scientific men.

Since our last meeting, the first volumes of Liebig and Kopp's very interesting annual report on the progress of Chemistry and the allied sciences have issued from the press. Full and complete as to Chemistry, it is a very excellent index to the recent papers on physical science in the periodicals, and supplies that kind of information so useful to a man, who, fully occupied with one engrossing pursuit, is anxious to know something of what is passing around him. Of the progress, however, of abstract mathematics it gives no account: there, however, there has been progress also. Indeed,

just as the increase of manufactures has always led to further efforts to perfect the beautiful machines, called tools, so the progress of physical science has at all times promoted the study of abstract mathematics. It was thus that the attempts to solve problems relating to attraction, heat, and electricity, led to the discovery of the most remarkable properties of definite integrals. To the fruits gathered from this branch of mathematics by Euler, Legendre, and other most illustrious analysts of past generations, our own contemporaries, both on the continent and in this country, have added results of the utmost value. Abroad, Jacobi, Cauchy, Dirichlet, Liouville and Catalan, have not only assigned the values of definite integrals previously unknown, but, what is of more consequence, they have established in relation to them several theorems of the greatest generality and elegance. Amongst ourselves, Mr. Ellis, Mr. Boole, Mr. Cayley, Mr. Thomson and Mr. Hargreave, have pursued the same track with distinguished success. Within the last two years the mathematicians of Germany have diligently cultivated the study of definite integrals, determining their values, investigating their properties, and employing them in the summation of infinite series.

The theory of elliptic integrals having lately formed the subject of an elaborate report laid before the British Association for the Advancement of Science, I need say little more in reference to the cultivation of it, than that the illustrious author of the *Fundamenta Nova* still labours at the building up of his theory; nor does he want the aid of fellow-labourers who have profited by his teaching. The volumes of Liouville's and Crelle's journals, for the last two years, contain articles on this subject by Guderman, Liouville, Mayer and Cayley. Mathematicians have made some curious applications of the theory of elliptic functions to the solution of problems in geometry: of these, the following is one of the most remarkable:—If a rectilinear polygon admits of being inscribed in one circle, and circumscribed about another, there exists an equation of condition between the radii of the two circles, and the mutual distance of their centres. Jacobi first pointed out the connexion between the problem of determining this condition, and that of dividing an elliptic function into as many equal parts as the polygon has sides. In a recent number of Crelle's Journal, Richelot has published an interesting paper, in which he shows how to derive the equation of condition in its rational and simplest form, from the formulæ which relate to the division of elliptic functions.

Geometry, both pure and analytic, has of late engaged much of the attention of foreign mathematicians. The general properties of surfaces relating to their tangent planes and normals, to their radii and lines of curvature, and to the shortest lines traced upon them, have been investigated afresh by various methods; old theorems have been brought into forms better suited for particular application, and many new ones have been arrived at. In the discussion of surfaces of the second order in particular, very interesting results have been obtained. The difficulties which attended the integration

of the differential equation of their geodetic lines having been at length overcome, the properties of these curves, and also of the lines of curvature, have been carefully investigated by the geometers of different countries. Abroad, the first important steps were made by Jacobi, Joachim, Stahl and Liouville. Profiting by their labours, Mr. Michael Roberts made the remarkable discovery that the lines of curvature of an ellipsoid are related to its umbilics, as a central conic section to its foci. Since then, Chasles, Liouville, Graves, Hart and others, have arrived at various theorems concerning the geodetic lines, and lines of curvature of surfaces of the second order.

In the theory both of surfaces and curves, considerable advances have been recently made by means of general methods of transformation. Thus, from theorems already established, new ones are derived without the need of an independent demonstration. Mr. William Roberts, Mr. Thomson and Mr. Cayley, have lately furnished methods for transformation, by the use of which new and interesting properties of curves have been established. The progress made in the evaluation of definite integrals has likewise led to many discoveries respecting the rectification of curves, and the quadrature of surfaces.

In ordinary algebra and the theory of equations, no very considerable advance has been made during the last two years. I ought not, however, to omit noticing a valuable paper, on Sturm's Functions, contributed by Mr. Cayley to Liouville's 'Mathematical Journal.' Crelle's Journal also contains some useful memoirs on continued fractions, infinite series, and the results of certain substitutions in functions of different degrees.

The theory of numbers has of late found favour in the eyes of mathematicians. Jacobi and other writers in France and Germany have given to the public several interesting papers on different subjects belonging to this department of mathematics; but of recent contributions to it, perhaps the most remarkable has been made in this country by Mr. Hargreave, in a paper "On Prime Numbers," published in the 'Philosophical Magazine.' Sir John Herschel has also very recently written on the same subject; his paper, as you will recollect, was read last Session, but is not yet published.

The theory of La Place's coefficients, so successfully treated by Mr. Hargreave and by Mr. Boole, has been lately made the subject of a memoir by Mr. Cayley, in which he has shown how to extend it to any number of variables. Neumann has also exhibited a method of developing, in a series proceeding according to La Place's functions, the distance between two points expressed by means of elliptic coordinates. In addition to the papers already enumerated, I observe that several relating to the solution of differential equations, the properties of particular series, factorials, faculties, and determinants, have been contributed to the principal scientific journals.

Before concluding this brief and necessarily imperfect review of the recent progress of mathematics, I cannot abstain from remarking, that two of the fields of research which promised best to reward

the labours of the mathematicians appear of late to have been cultivated almost exclusively by our own countrymen,—I refer to the Calculus of Operations and the Theory of Imaginaries. Two important papers on the former subject have lately appeared in our Transactions, and have earned for their authors the Medals of the Society. In the latter field, Mr. De Morgan, Mr. John Graves, Mr. Charles Graves, and Mr. Kirkman, have succeeded in obtaining many curious and interesting results; but the most valuable seems to be those which have been arrived at by Sir William Rowan Hamilton in his Theory of Quaternions: his late application of it to the geometry of three dimensions, and to questions in optics and astronomy, evidently prove the power of this Calculus as an instrument of invention and research.

I regret to say, that since our last Meeting we have lost many eminent men; the obituary memoirs, however, seem to me to call for no particular remark, with one exception, that of the distinguished chemical philosopher Gay-Lussac. That memoir has been prepared by M. Biot, and I am sure you will concur with me in thinking, that in performing that duty he has conferred a great obligation on the Royal Society and on all men of science; the memoir will be read, uninjured by translation, just as we have received it.

Col. Sabine,

I have the honour of presenting to you the Copley Medal in charge for Professor Hansen. It has been awarded to him for his discoveries in Physical Astronomy.

To have contributed even a little to the advancement of that science, as it exists at the present day, would have been no mean achievement. Hansen has done much more; his contributions are of a very high order. Previously to the appearance of Hansen's memoirs, two methods had been employed in the determination of the planetary perturbations. One of these, known by the name of the method of variation of elements, besides being exceedingly beautiful in theory, is peculiarly well adapted to the determination of the secular variations and the inequalities of long period. It has the disadvantage, however, that the number of elements whose perturbations we are thus required to calculate, is twice as great as that of the coordinates, the perturbations of which are the final objects of investigation; and also that the former perturbations are frequently much greater than the latter. In order to take into account the square of the disturbing force, which is quite necessary in the present state of astronomy, we must find the change introduced into the perturbation of each element of the disturbed planet, by the perturbations of each several element both of the disturbed and disturbing planets. Consequently, unless we content ourselves with the selection of a few of the most important terms, the calculations become extremely long and laborious.

In the second of the methods above alluded to, the perturbations

of the planet's coordinates are found at once by the integration of the proper differential equations. But this method, though perhaps preferable to the former for calculating the inequalities depending on the first power of the disturbing force, is inapplicable, or at any rate seems as yet not to have been applied to the calculation of those which depend on the square and higher powers of that force, so that it cannot be regarded as affording a complete solution of the problem.

In Hansen's method, the perturbations are applied to the *mean* longitude and to the *logarithm* of the radius vector. The disturbed mean longitude, combined with *invariable* elements, gives the true longitude in the orbit, and the logarithm of the true radius vector is found by adding its perturbations to that value which corresponds with the disturbed mean longitude and the same invariable elements as before. The perturbations of latitude are determined in a manner more analogous to the ordinary method of variation of elements. Thus, with respect to the longitude and radius vector, Hansen avoids the inconvenience of having to calculate the perturbations of twice as many quantities as are finally wanted, while at the same time his formulæ take into account all powers of the disturbing force.

Hansen finds also that the series which expresses the perturbations of the *mean* longitude is more convergent than that in ordinary use which gives the perturbations of the *true* longitude; and Mr. Adams found this to be the case in his investigation of the disturbances of Uranus, the use of the perturbations of the *mean* longitude, instead of those of the *true*, having been attended with considerable advantage.

Astronomers have long seen the convenience of applying the inequalities of long period to the *mean* longitude of the planet, and similar advantages, though not of so marked a character, follow from applying all the inequalities in the same manner.

Only a part of Hansen's investigations respecting the perturbations of bodies moving in orbits of great excentricity and inclination has yet appeared. In this part he treats of the case where the distance of the disturbed body from the sun is always less than that of the disturbing. This is perhaps the most important case, as it includes the disturbances of the minor planets and of Encke's comet produced by Jupiter, and the planets exterior to it.

An example is given of the application of the formulæ to the disturbances of Encke's comet caused by Saturn. In this case the method succeeds perfectly, and there is no doubt of its applicability when the distance of the disturbed and disturbing bodies from the sun never become very nearly equal to each other. If these distances ever approach very closely to equality, or if one of them is sometimes greater and sometimes less than the other, the calculations become much more complicated. Hansen's lunar theory, "Fundamenta nova investigationis orbitæ lunæ," contains the most complete view of the principles of the method first adverted to above; but the numerical results of his formulæ have not yet been published.

His discovery of the long inequalities in the moon's motion, caused by the action of Venus, is one of the most important that has ever been made in the lunar theory; the calculations, however, have not yet appeared in a complete form.

Benjamin Collins Brodie, Esq.,

I am most happy to present you a Royal Medal for your investigations 'On the Chemical Nature of Wax.' Independent of the important addition to our knowledge of the true nature of wax, Mr. Brodie has succeeded in discovering two new alcohols of the fatty acids—Cerotin and Melissin. These bodies stand in the same relation to cerotic and melissic acids, that wood-spirit and alcohol do to formic and acetic acid.

The addition of a new alcohol to our knowledge has been pronounced by Dumas a contribution to the department of organic chemistry as important as that of a new metal is to inorganic chemistry; both serving as starting-points for future researches.

Professor Quain,

I have much pleasure in presenting you, in charge for Professor Graham, a Royal Medal for his papers 'On the Motion of Gases.' Mr. Graham's researches have disclosed the existence of a hitherto unobserved property in gaseous bodies; they relate to the flow of gases and vapours through very long capillary tubes, into a vacuous or partially vacuous space; the rapidity of motion varies in the different gases, but the rate of motion has not as yet been found connected with any other physical property. A new series of constants of high interest have been obtained by these investigations.

Colonel Sabine,

I have now the gratification of presenting to you the Rumford Medal to transmit to M. Arago, whose long and brilliant career, as one of the greatest discoverers of the age in Physical Optics, has excited the admiration of all men of science:—the discoverer of coloured polarization, of rotatory polarization, of the polarization of the sky, and of many other important facts embodied in modern works on Light. The Medal has been awarded to him for his 'Experimental investigations on Polarized Light,' the concluding memoirs on which were communicated to the Academy of Sciences of Paris during the last two years.

The President then called upon Mr. Christie to read the biographical notices of some of the deceased Members, which he handed to him.

Henri-Marie Ducrotay de Blainville was born at Arques in Normandy, September 12, 1777, of an ancient and respectable

family, and received his early education at the Military School of Beaumont-en-Auge. On the dissolution of this establishment at the Revolution, he was sent to Paris to complete his education at the "Ecole du Génie, Mathématiques et Dessin," and was exempted from the conscription of 1798 by reason of an accident in the right arm followed by partial anchylosis. He accordingly remained in Paris, occasionally attending lectures on the Natural Sciences, and studying painting in the Drawing Academy of Vincent. He had attained the age of twenty-seven, still undecided as to a profession, when, having listened to an eloquent lecture by Cuvier, at the College of France, M. de Blainville left the theatre with a determination to pursue the science of Comparative Anatomy. For this purpose, he, by the advice of M. Duméril, entered himself as a student in the School of Medicine; and after devoting three years to the usual studies, took his degree as Doctor of Medicine in 1808; the subject of his inaugural dissertation being founded on some experiments on the influence of the eighth pair of Nerves in Respiration.

The ardour of M. de Blainville in the pursuit of what had now become his absorbing science, and his skill as a draughtsman, procured for him the especial attention of Baron Cuvier, who, after employing him as a practical anatomist and artist at a salary of 2000 francs per annum, confided to him the delivery of a part of his Course of Lectures on Zoology, at the College of France; and he soon afterwards obtained, by a successful *concours*, the Chair of Zoology and Physiology at the Faculty of Sciences, on which occasion he defended his well-known Thesis 'On the Natural Affinities of the *Ornithorhynchus paradoxus*.'

At the restoration of Louis XVIII. the opportunity presented itself to M. de Blainville, through his family connexions and friends, of obtaining office in the administration of the newly-established legitimate government, to the principles of which M. de Blainville was sincerely attached. But he resisted the temptation, and remained faithful to his scientific pursuits. He took advantage of the peace to visit the Museums of England in 1816, and made many drawings of rare Mollusca in the British Museum, and of anatomical specimens in that of the Royal College of Surgeons. Several Memoirs inserted in the Bulletin of the Philomathic Society testify to the ardour with which he availed himself of his short sojourn in this country; and he soon after collected and methodized his researches into the organization of the invertebrate animals in the form of two extensive Articles in the 'Dictionaire d'Histoire Naturelle,' afterwards published as distinct works, one entitled 'Manuel de Malacologie,' the other 'Manuel d'Actinologie.' A vast number, upwards of 180, Memoirs in the 'Bulletin de la Société Philomathique,' the 'Journal de Physique,' the 'Annales' and 'Mémoires du Muséum,' attest M. de Blainville's active researches in all branches of Zoology. He published in 1822 one volume of a Course of Lectures 'On the Organization of Animals, or Principles of Comparative Anatomy,' a work which was never completed.

M. de Blainville was elected a member of the Academy of Sci-

ences in the Institute of France, as the successor of M. de Lacépède; and soon after, in 1830, was appointed to the Chair of the Natural History of the *Mollusca* and *Radiata* in the Jardin des Plantes, on the retirement of Lamarck. Two years afterwards, on the lamented demise of Baron Cuvier, M. de Blainville was nominated his successor in the Professorship of Comparative Anatomy at the Jardin des Plantes. This Professorship placed him at the head of the famous Museum of Comparative Anatomy, the formation of which had been one of the Herculean labours of Cuvier's great career; and M. de Blainville availed himself of it to commence his great work 'On the Osteography of the Vertebrate Classes,' which contains the most beautiful and accurate figures of the skeletons of a large proportion of the Mammalian Class. Twenty-three fasciculi of this work had been published, and M. de Blainville had corrected the proof-sheets of the twenty-fourth fasciculus, 'On the Osteography of the Camel-Tribe,' on the morning of his death. He had taken his place in a railway carriage for Rouen on the 1st of May, 1850; and was found, on the arrival of the train there, in a state of apoplectic insensibility. He was removed to the waiting-room; an attempt was made to bleed him, but in vain, and he soon after expired, at the age of seventy-three.

He was elected a Foreign Member of the Royal Society in 1832.

Gay-Lussac (Louis Joseph), l'un des physiciens et des chimistes les plus distingués de notre tems, naquit le 6 Décembre 1778, à St. Léonard, petite ville du département de la haute Vienne, où son père exerçait la charge de Procureur du Roi. La révolution de 1789, qui éclata lorsqu'il sortait de l'enfance, contraignit sa famille à le garder près d'elle, durant les années où il aurait pu recevoir une éducation classique, dans des tems meilleurs. Ce ne fut qu'en 1795, lorsqu'il avait déjà 16 ans accomplis, qu'un peu de sécurité étant revenue, ses parens se décidèrent à l'envoyer à Paris pour y faire quelques études, et se préparer aux examens d'admission de l'École Polytechnique. Malheureusement, une grande disette étant survenue, M. Censier, le chef de l'établissement où il était entré, se vit forcé de congédier tous ses pensionnaires. Mais les rares dispositions de Gay-Lussac, et l'aménité de son caractère, lui ayant inspiré une vive affection, il le garda, plutôt comme un fils que comme un élève. Grâce à cette heureuse association des qualités morales avec les dons de l'intelligence, qui le distingua toujours, il fut en état d'être admis à l'École Polytechnique le 27 Décembre, 1797. Il en sortit le 22 Novembre, 1800, dans les premiers rangs du service des ponts et chaussées, où les meilleurs élèves se pressaient alors. Avant de raconter ses nombreux succès, dans la carrière scientifique, nous n'avons pas cru inutile de montrer les difficultés qu'il a du traverser, pour s'en ouvrir l'accès.

Berthollet était alors professeur de chimie à l'École Polytechnique. Il remarqua ce jeune homme si bon, si zélé, si intelligent. Il en fit son répétiteur; et bientot le fixa près de lui, dans sa délicieuse retraite d'Arcueil, où, entouré de tous les instrumens du physicien et du chimiste, il travaillait à son grand ouvrage sur la statique chimique,

éclairé, soutenu, par les entretiens journaliers de son ami Laplace, dont, un peu plus tard, la résidence touchait la sienne. Ce fut sous l'influence de ces deux hommes que Gay-Lussac prit son essor.

Ils dirigèrent d'abord son jeune talent vers ce champ de recherches commun à la physique et à la chimie, que le génie inventif de Dalton avait commencé à explorer avec une activité si féconde, dans le mémoire intitulé *Experimental Essays, &c.*, qu'il publia en 1801 *. C'était en effet, à cette époque, le sujet de travail qui pouvait être le plus fructueux et le plus utile, pour fixer une foule de données dont l'emploi revient sans cesse, dans les recherches expérimentales, et qui étaient alors ignorées, ou imparfaitement établies. Obéissant à cette inspiration, Gay-Lussac fit, dans la même année, 1801, son premier mémoire sur la dilatation des gaz et des vapeurs †; puis, sans s'arrêter, une foule de recherches sur le perfectionnement des thermomètres et des baromètres, sur la tension des vapeurs, leur mélange avec les gaz, l'appréciation de leur densité, l'évaporation, l'hygrometrie, et la mesure des effets capillaires. Cela le conduisit jusqu'en 1803. Une occasion rare s'offrit alors, d'utiliser cet ensemble de connaissances physiques qu'il avait acquises. Il avait été chargé de faire, avec un de ses amis, une ascension aérostatique, pour savoir, s'il était vrai que la force magnétique cesse d'agir hors du contact de la masse terrestre, comme on l'avait annoncé. Ils constatèrent, qu'au contraire, elle se conservait sans affaiblissement notable, dans l'espace libre, jusqu'à 4000 mètres d'élévation. Mais leur ballon s'était trouvé trop faible pour les porter plus haut, tous deux ensemble. Alors Gay-Lussac fit seul une deuxième ascension, dans laquelle il s'éleva jusqu'à la hauteur de 7000 mètres, la plus grande qu'aucun homme eut jamais atteint. Il confirma l'observation déjà faite sur la persistance de la force magnétique; il rapporta de ces hautes régions, de l'air, qui, analysé, se trouva avoir la même composition qu'à la surface de la terre; et il recueillit en outre une série de déterminations importantes, sur le decroissement régulier des pressions, des températures, de l'humidité atmosphérique, dans tout l'intervalle de hauteur qu'il avait parcouru ‡. Ce dernier succès, venant, pour ainsi dire, couronner toutes ses précédentes recherches, acheva de lui donner, à très juste titre, la réputation d'un physicien consommé. Effectivement, si l'on se reporte à l'époque de ces travaux, on ne saurait y méconnaitre un progrès notable sur tout ce qui avait précédé. Les opérations, les appareils, ont un caractère de simplicité ingénieuse, qui distingua toujours Gay-Lussac. On y remarque une intention générale d'exactitude plus grande, et des résultats relativement plus précis. Toutefois, du point de vue où nous pouvons envisager aujourd'hui ces investigations, il est évident que le sujet en était trop complexe, pour être pénétré à fond par des procédés d'expérience aussi restreints. Il faut y appliquer des appareils d'une conception plus générale, et d'un mécanisme plus sur, comme plus varié, pour embrasser l'ensem-

* Mémoires de la Société Philosophique de Manchester, tome v. partie ii. page 535.

† Annales de Chimie, tome xliii. page 137.

‡ Annales de Chimie, tome lii. page 75.

ble de toutes les circonstances qui y concourent, pour suivre isolement chacune dans ses détails propres, et pouvoir en recomposer l'effet total. Enfin, il faut en éxiger une précision bien plus grande, pour apprécier, non pas seulement ce que l'on pourrait appeler le gros des phénomènes, mais aussi, et surtout, leurs particularités spécifiques, qui en établissent le caractère essentiel et intime. Ainsi, le coefficient de dilatation des gaz permanens et des vapeurs, trouvé par Gay-Lussac, était, à la vérité, plus exact que celui de Dalton; mais il était encore loin de la réalité*. En outre, comme le physicien Anglais, Gay-Lussac la cru pareil pour tous ces fluides, tandis qu'il est sensiblement différent; et il la supposé aussi constant pour chacun d'eux, tandis qu'il varie avec les pressions et les températures. Or, toutes minimes que ces variations nous apparaissent, dans les amplitudes restreintes où nous pouvons les observer, la connaissance seule de leur existence a une importance capitale; puisqu'elle change toutes les idées que l'on avait pu concevoir sur la constitution des fluides aëriformes, tant qu'on en faisait abstraction.

Peut-être Gay-Lussac comprit-il ce qui lui manquait, ce qui manquait aussi à son tems, pour suivre plus loin ce genre de recherches. Car, tout en faisant un heureux et habituel usage des notions physiques qu'il y avait acquises, on ne le voit plus y revenir; et depuis la formation de la société d'Arcueil, en 1807, il s'attacha presque exclusivement à des recherches de chimie; ce qui forme, pour ainsi dire, la seconde phase, et la plus brillante comme la plus durable, de ses travaux.

* Soit 1 le volume qu'une masse de gaz sec occupe à la température de la glace fondante, ou 0° cent, sous la pression moyenne de l'atmosphère à la surface de la terre. Si cette masse est portée à la température de 100° Cent, *sous la même pression*, son volume deviendra :—

selon Dalton ....1·3912
selon Gay-Lussac ....1·3750

Ces déterminations sont toutes deux fautives, sous plusieurs rapports. Elles le sont, en premier lieu, dans la supposition de généralité que leurs auteurs y attachaient, puisque le coefficient de dilatation des gaz varie avec leur nature chimique, étant évalué dans des conditions pareilles. En second lieu, elles le seraient encore pour un même gaz, l'air atmosphérique par exemple, pour n'y avoir pas distingué les deux cas du problême, savoir : celui où le volume se dilate, sous une pression constante; et celui où on le maintient constant, sons une pression variable, l'intervalle de température parcouru étant pareil. Dans ce deuxième mode d'expérimentation, le coefficient de dilatation se conclut de la force élastique par la loi de Mariotte, qui est suffisamment exacte pour ces réductions. En considérant ainsi un volume d'air atmosphérique sec, pris d'abord à la température 0°, sous la pression 0^{m}·76, puis porté à la température de 100°, le coefficient de dilatation qui lui est propre, entre ces limites de températures, a été trouvé :—

par M. Regnault (le volume variant sous une pression constante) ....0·367
(le volume étant maintenu constant, et la pression étant variée) 0·3665
par M. Magnus (le volume étant maintenu constant, et la pression étant variée) 0·3665

D'après ces derniers résultats, qui offrent toutes les garanties d'exactitude, on voit que le nombre donné par Gay-Lussac était trop fort, et celui de Dalton plus éloigné encore de la vérité dans le même sens. On doit à Rudberg, d'avoir fait connaitre aux expérimentateurs le défaut du coefficient de Gay-Lussac, jusqu'alors adopté universellement, sans qu'on l'eut vérifié. Il le réduisit à 0·3645, valeur plus rapprochée de la vérité, mais un peu trop faible; tant les dernières décimales des déterminations physiques sont difficiles à obtenir avec une entière sureté.

Il ne serait pas possible de mentionner ici tous ses mémoires. Ils se suivent, presque sans interruption, dans les volumes des *Annales de Chimie et de Physique*, pendant plus de trente années. Partout, jusque dans les plus simples notes, on apperçoit ses qualités distinctives, un esprit droit, lucide, des conceptions nettes, et la fermeté de jugement qui le retient toujours dans l'expression stricte des faits. On les reconnaîtrait à ces caractères, sans qu'elles fussent signées. Pour montrer le rang élevé où il s'est placé, comme chimiste, nous rappellerons seulement ceux de ses travaux qui, par leur nouveauté, leur importance, ou les progrès ultérieurs dont ils ont été l'origine, nous semblent mériter le plus d'être signalés.

Celui que nous mentionnerons d'abord, lui fut suggéré par une observation qui remonte presque aux débuts de sa carrière chimique. En 1804, M. Alexandre de Humboldt, déjà célèbre par son voyage aux régions équinoxiales, avait fait au jeune Gay-Lussac l'honneur de se l'associer pour des recherches d'eudiométrie. Ils reconnurent que, dans la formation de l'eau, 100 parties en volume de gaz oxygène, se combinent, par la combustion, avec un volume de gaz hydrogène si proche d'être égal à 200 parties, que l'on ne pouvait pas répondre expérimentalement de la différence*. La tendance de ces nombres vers une limite simple, frappa Gay-Lussac. Il soupçonna immédiatement que le rapport exact de 1 à 2 était le véritable, et que cette simplicité pouvait bien être un fait général, analogue, pour les volumes, à celui que Dalton avait découvert, pour les proportions de poids suivant lesquelles les corps forment leurs combinaisons de différens ordres. Ayant suivi silencieusement cette idée, avec persévérance, dans tous les cas d'application qu'il put trouver, il la présenta comme certaine quatre ans plus tard, à la fin de 1808, non sans quelque crainte de la part de ses amis†. Le résultat, tel qu'on peut l'énoncer aujourd'hui, consiste en ce que : *Lorsque deux gaz se combinent, leurs volumes ont entre eux des rapports numériques simples ; et le volume du composé qu'ils forment, étant considéré à l'état de gaz, présente aussi un rapport simple, avec la somme des volumes des gaz qui sont entrés dans la combinaison.* Cette *loi des volumes* est devenue une des plus utiles que l'on ait trouvées en chimie, bien qu'il ait fallu quelque tems pour qu'on en sentit la valeur. L'énoncé que nous venons d'en donner, ne diffère de celui de Gay-Lussac, que par une étendue et une précision d'application, dues aux progrès du tems. La simplicité des rapports qu'elle suppose n'existe, et ne peut évidemment exister, qu'autant que l'on néglige les inégalités de dilatation des gaz, qui, étant presque toujours insensibles dans les expériences de chimie habituelles, restreignent, plutôt théoriquement que pratiquement, son usage. Il ne faut pas mettre à la charge de Gay-Lussac les systêmes que l'on a voulu y rattacher, en ne tenant pas compte de cette circonstance ; car il ne les a jamais acceptés. Les spéculations hypothétiques répugnaient souverainement à la nature de son esprit.

Il dut se décider à faire connaître cette loi des volumes, sans plus de retard, quand il apperçut les utiles applications qu'elle avait déjà,

* Annales de Chimie, tome liii. page 248.

† Mémoires de la Société d'Arcueil, tome ii. page 207.

dans une série de recherches chimiques, dont il s'était activement occupé avec M. Thénard, pendant tout le cours de cette même année 1808. La fin de la précédente, 1807, venait d'être illustrée par une grande découverte ; continuation heureuse des études patientes faites par Hisinger et Berzelius, sur le pouvoir du courant voltaique pour désunir les élémens des corps composés. En soumettant les effets de ce pouvoir à des expériences nombreuses et précises, les deux chimistes Suédois avaient constaté la faculté générale qu'il a, non seulement de séparer les principes des combinaisons, mais aussi de les transporter à des pôles contraires, par exemple l'oxygène des oxides, et des acides, au pôle vitré ; le principe complémentaire, au pôle résineux. Durant l'année 1806, Davy s'était profondément occupé de ces phénomènes de transport. Concevant toute leur importance, il les avait multipliés, variés, et il avait fait mille efforts pour fixer les conditions de leur accomplissement. Il les reprit encore l'année suivante, avec des appareils voltaiques plus puissants, et il parvint à décomposer ainsi la potasse et la soude. Il en avait extrait des substances d'apparence métallique, malléables, éminemment conductrices de l'électricité. D'une vue hardie et sure, il les signala d'après ces caractères, comme deux métaux simples, qu'il nomma le *potassium* et le *sodium*. Les deux alcalis en étaient des oxides. Pendant que le grand chimiste Anglais poursuivait avec ardeur les innombrables effets de ces nouvelles substances, comme agens de décomposition des autres corps, Gay-Lussac et M. Thénard se jettèrent ensemble dans cette voie, à sa suite. Ils découvrirent, et annoncèrent bientôt (7 Mars, 1808) un procédé chimique, qui fournissait les nouvelles substances beaucoup plus abondamment que les appareils voltaiques.* Ils pûrent ainsi étudier leurs caractères propres, et leurs actions sur les autres corps, avec plus de facilité, de généralité, de précision. Dans la multitude de ses premières tentatives, Davy avait apperçu des indices évidens, mais presque insaisissables, de la décomposition de l'acide borique, qu'il avait seulement signalés, sans pouvoir les suivre, pressé par tant d'autres objets. Mettant à profit les agens actifs qu'ils s'étaient procurés, les deux expérimentateurs Français attaquèrent cet acide, en le chauffant avec le potassium. Ils lui enlevèrent ainsi son oxygène, isolèrent son radical, qu'ils appelèrent *le bore*, et le reproduisèrent aussi, par synthèse.† Davy obtint bientôt après des résultats pareils,

* Ils firent arriver la potasse et la soude fondues, au contact du fer incandescent, maintenu à une très haute température. Voyez Recherches Physico-chimiques, faites par MM. Gay-Lussac et Thénard, tome i. page 74 et suiv.

† La première annonce de ce procédé, et de ses résultats, fut communiquée à l'Institut par une note, lue, au nom de Gay-Lussac et de Thénard, le 20 Juin 1808. Elle fut imprimée immédiatement, dans le bulletin de la Société Philomatique, pour le mois de Juillet de cette même année, page 173. Gay-Lussac était alors gravement malade d'une explosion qui avait failli l'aveugler. Davy annonça des tentatives du même genre, mais moins avancées, dans un mémoire daté du 30 Juin, qui est inséré aux transactions philosophiques de 1808, voyez page 343, note. Les résultats définitifs des deux chimistes Français ont été consignés au Moniteur, dans les Nos. des 14 et 15 Novembre 1808. Ceux de Davy le furent dans sa leçon Bakérienne, datée du 15 Décembre, qui est insérée aux transactions philosophiques de 1809 ; voyez page 75. Voyez aussi pages 41 et 42, le passage où il reconnaît avec une entière sincérité qu'il s'est servi du procédé (happy method) de Gay-Lussac et Thénard, pour la préparation du potassium, préférablement à l'action voltaique.

s'étant pourvu désormais de potassium par la méthode chimique, dont il reconnut noblement les avantages. Pendant cette année 1808 et les suivantes, les travaux incessans du savant Anglais furent, pour Gay-Lussac et Thénard, le sujet fécond d'une vive et continuelle concurrence. Il ne fallait pas moins qu'une rivalité aussi active, pour mettre si vîte au jour tous les trésors que renfermait sa découverte. La lutte s'établissait, au profit de la science, dans les idées, comme dans les faits. Ainsi, une dissidence d'un moment s'éleva, sur la nature des substances que Davy avait signalées. Les effets qu'on en obtenait pouvaient se représenter à peu près aussi bien, en admettant qu'elles fussent, comme il le croyait, des métaux simples, qui formaient la potasse et la soude par leur combinaison avec l'oxygène ; ou en supposant qu'elles fussent des hydrures de ces bases alcalines, totalement dépouillées d'eau. Cette dernière interprétation semblait se rattacher mieux que l'autre aux idées antérieurement admises en France. Sous cette influence, Gay-Lussac et Thénard l'embrassèrent d'abord ; mais une exploration plus étendue des faits la leur fit ensuite abandonner, pour revenir au sentiment de Davy, qui est aujourd'hui adopté universellement dans toute l'extension qu'il lui avait donnée dès l'origine ; les expériences ultérieures l'ayant pleinement confirmé*.

Une alternative d'interprétation analogue s'offrit encore à leur esprit, quand eux, et Davy également, se servîrent du potassium, pour essayer de décomposer les deux corps que l'on appelait, à cette époque, l'acide muriatique, et l'acide muriatique oxygéné. Mais, quoique la question fut particulière, elle avait une importance capitale pour la théorie de Lavoisier, jusqu'alors universellement admise. Dans cette théorie, l'acide muriatique devait être le premier dégré d'oxidation d'un radical inconnu ; et l'acide muriatique oxygéné en était le deuxième. En combinant ce second corps, à l'état de gaz sec, avec l'hydrogène gazeux, on reformait le premier, qui, alors, devait contenir de l'eau. Or, aucun procédé, aucun agent chimique, ne réussissait à y faire constater la présence des deux élémens de cette eau, qu'on y supposait ; et l'on n'en pouvait jamais dégager qu'un seul, l'hydrogène. D'une autre part, on ne parvenait pas à extraire, du gaz muriatique oxygéné sec, la moindre trace d'oxygène. Après une active concurrence de recherches expérimentales, variées des deux côtés, avec une égale persévérance, Gay-Lussac et Thénard apperçûrent que l'on pouvait éluder la difficulté, en intervertissant les relations théoriques des deux corps ; c'est à dire, en considérant celui qu'on appelait oxygéné comme une substance simple, qui, par sa combinaison avec l'hydrogène, formait l'autre acide†. Cette nouvelle vue faisait brêche à la théorie de Lavoisier, où l'on suppose que l'oxygène est le seul principe acidifiant. Ils se bornèrent, trop prudemment peut-être, à la présenter comme également compatible avec les faits ; et, retenus par la considération des grands changemens qu'elle nécessitait, dans l'ensemble de leurs rapports, jusqu'alors admis, ils continuèrent d'em-

* Voyez la discussion de ce point de théorie, Recherches Physico-Chimiques, tome ii. page 218 et suiv.

† Mémoires de la Société d'Arcueil, tome ii. page 358. Lu à l'Institut le 27 Février 1809.

ployer l'ancienne interprétation comme préférable. Davy n'était pas astreint aux mêmes réserves. Après beaucoup de tentatives, faites dans l'ancienne voie, il se prononça exclusivement pour l'idée que l'acide muriatique oxygéné était une substance simple, et il lui donna le nom de *chlorine*, en français *chlore*, qu'on lui a conservé*. Ce choix était conforme aux règles de la philosophie expérimentale, n'exigeant qu'une seule hypothèse, celle de la simplicité du chlore, tandis que l'autre interprétation en exigeait trois, savoir: la présence de l'oxygène dans un des corps; de l'eau dans l'autre; et, en outre, l'existence du radical inconnu. Mais l'initiative du doute, et l'énoncé de l'alternative, appartiennent, par leur date, aux deux chimistes Français, comme Davy l'a reconnu lui-même†. Or, si l'on considère la grande autorité des opinions qui régnaient autour d'eux, on trouvera qu'il a fallu beaucoup de force et d'indépendance de jugement, pour s'en affranchir, même jusque là. C'est ce que des témoins, encore vivans, pourraient attester.

Les vues que cette controverse avait fait naître, devinrent très utiles à Gay-Lussac, lorsque vers la fin de 1813, son attention se porta sur une nouvelle substance, qu'un manufacturier Français, M. Courtois, avait découverte dans les lessives de Varecks. Le 6 Décembre, il lut à l'Institut un court mémoire, dans lequel il établissait ses propriétés distinctives, et la désignait, comme substance simple, par le nom d'*Iode*, en Anglais *Iodine*, qui lui est resté. Ayant reconnu, dès ces premières épreuves, son analogie avec le chlore, il l'avait engagée aussitôt, dans une multitude de combinaisons parallèles, où elle porta des caractères semblables. Il l'avait combinée de même avec l'hydrogène, et en avait obtenu ainsi un acide puissant, qu'il appela *Hydriodique*, s'autorisant de ce nouveau fait, pour se rallier ouvertement au mode d'interprétation qu'il avait voulu d'abord adopter, dans le cas du chlore. Quinze jours après, le 20 Décembre, il annonça qu'il était parvenu à combiner aussi l'iode avec l'oxygène, d'ou résultait un deuxième acide, qu'il appelait l'*iodique*. Ceci pouvait paraître un apperçu contestable; il le confirma plus tard, par une autre voie. Dans l'intervalle de ces deux communications, Davy se trouvait à Paris, son génie lui ayant servi de titre à un passeport exceptionnel. On vit alors un bel exemple d'émulation scientifique. On lui avait donné quelque peu de la nouvelle substance. Il en fit des essais en petit, avec cette adresse ingénieuse, qui lui faisait trouver, dans les moindres objets, des instrumens d'expérimentation. A' la prière de ses amis, au nombre desquels étaient ses émules, il consigna le résumé de ses observations, dans une note, qui fut lue à l'Institut le 13 Décembre, après la première, et avant la seconde communication de Gay-Lussac. Tous deux, depuis, conti-

* Researches on the oxymuriatic acid, &c., Philos. Trans. for 1810, p. 231. Lu à la Société Royale le 12 Juillet 1810. Bakerian Lecture. Phil. Trans. for 1811, lu à la Société Royale le 15 Novembre, 1810.

† Researches on the oxymuriatic acid, &c. Philosoph. Trans. for 1810, page 237. Voyez aussi, dans ce même mémoire, page 232, la citation faite par Davy, des recherches de Gay-Lussac et Thénard, publiées dans le 2e vol. de la Société d'Arcueil, où l'initiative de la nouvelle hypothèse est consignée.

nuèrent à s'occuper de ce sujet, pendant l'année suivante, avec une égale activité d'esprit, mais dans des conditions de travail bien différentes. Davy, devenu riche par un mariage récent, se rendait avec sa femme en Italie. Quelques instrumens de précision, et de manipulation, quelques réactifs chimiques bien purs, les plus indispensables, lui composaient un laboratoire portatif, qui le suivait partout, et lui suffisait. Il n'avait à sa disposition qu'une petite quantité d'iode, et ne pouvait donner aux expériences que les momens de loisir d'un voyage d'agrément: mais sa pensée y était toujours. Des trois mémoires qu'il adressa à la Société Royale, au sujet de l'iode, le premier est daté de Paris, le second de Florence, le troisième de Rome*. Ce dernier est du mois de Février, 1815. Il contient la découverte de l'acide iodique, à l'état solide et cristallisé, tandis que Gay-Lussac ne l'avait obtenu qu'en dissolution dans l'eau, ou en combinaison avec des bases, de manière à en donner toutefois l'analyse exacte. Du reste, par une conséquence naturelle, ces mémoires de Davy offrent une riche collection de faits détachés, habilement vus, plutôt qu'un travail d'ensemble. Gay-Lussac, mieux pourvu de matière, d'instrumens, et de tems, effectua ce travail dans les sept premiers mois de 1814†. Guidé par l'analogie qu'il avait reconnue entre le chlore et l'iode, il développa savamment et patiemment ce parallèle; la suivit dans toutes ses combinaisons, acides, salines, métalloides, éthérées, dont il assigna la composition; et il fixa toutes ses propriétés spéciales, si exactement, que l'on a pu seulement, depuis, étendre les résultats qu'il avait obtenus, ou perfectionner les procédés qu'il avait employés, sans rien trouver à reprendre à ses déterminations. E'tant parvenu à extraire l'acide iodique, des iodates, le même sentiment de correspondance le conduisit à extraire pareillement l'acide chlorique, des chlorates, d'où on ne l'avait pas encore retiré; et il en donna l'analyse exacte en proportions de poids, ainsi que de volumes. Son mémoire, inséré au tome 91 des Annales de Chimie, présente un remarquable ensemble de toutes les connaissances physiques et chimiques, appliquées à l'étude d'un nouveau corps, avec une sureté de jugement, et une finesse de tact, qui ne laissent rien d'incertain ou d'inexploré. Il est aussi complet et parfait qu'un travail chimique peut l'être, à son temps donné. C'est là que Gay-Lussac présenta le premier exemple de l'emploi qu'on peut faire de *la loi des volumes*, pour conclure, par induction, la densité des vapeurs des corps, que l'on ne sait pas vaporiser matériellement. Il se servit de cette méthode pour calculer la densité de la vapeur de l'iode qui n'était pas encore connue, et l'expérience a confirmé depuis cette détermination, si hardie alors.

Un an plus tard, en 1815, Gay-Lussac mit le sceau à sa réputation de chimiste, par la découverte de l'azoture de carbone, ou *cyanogène*. Indépendamment d'une multitude de faits nouveaux qu'elle a donnés,

* Transactions Philosophiques pour 1814, page 74, daté de Paris, 10 Décembre 1813, lu à la Soc. Re. 20 Janvier, 1814; même volume, page 487, daté de Florence 23 Mars, 1814, lu à la Soc. Re. 16 Juin 1814. Trans. Philos. pour 1815, page 203, daté de Rome, 10 Février, 1815, lu à la Soc. Re. 20 Avril 1815.

† Son mémoire fut lu à l'Institut le 1er Aout 1814.

et de la lumière qu'elle a jettée sur beaucoup de points jusqu'alors obscurs, cette découverte a été d'une haute importance pour la science chimique, sous deux rapports. D'abord, parcequ'elle a offert le premier exemple d'un corps composé, qui porte, et garde, dans ses combinaisons, les caractères de simultanéité que l'on avait cru jusqu'alors appartenir aux substances réputées simples; en outre, parceque, venant après celle de l'iode, et de l'hypothèse faite sur la simplicité du chlore, elle acheva de montrer avec évidence que l'oxygène n'entre pas comme élément nécessaire, dans la composition des corps qui possèdent les propriétés d'un acide ou d'un sel. Gay-Lussac étudia ce nouveau produit, dans toutes ses phases de combinaison et d'isolement*: il détermina toutes ses propriétés physiques et chimiques, immédiates. Il définit rigoureusement sa composition par deux procédés d'analyse précis, et divers; d'abord en le faisant détonner dans l'eudiomètre de Volta; puis en le brûlant par le bioxide cuivre; ce qui était un perfectionnement considérable de la méthode qu'il avait antérieurement imaginée avec M. Thénard, pour analyser les matières organiques par voie de combustion. Il développa alors toutes les particularités de constitution, tant du cyanogène même, que de ses combinaisons, dans leurs rapports avec la loi des volumes qu'il avait découverte. On retrouve, dans ce beau travail, toutes les excellentes qualités d'esprit qu'il avait montrées dans l'étude de l'iode. Mais la sagacité et la sureté avec lesquelles il sut saisir les caractères si imprévus du nouveau produit qu'il avait formé, complétèrent l'idée que l'on avait conçue de son mérite, en y ajoutant la gloire d'un inventeur pénétrant et prudent.

Ici il donna le second exemple pratique, de la loi des volumes employée pour calculer la densité des vapeurs des corps non vaporisables. Les nombreuses vérifications qu'il en avait faites sur les composés divers des corps gazeux, lui ayant inspiré toute confiance dans ses applications, il eut la hardiesse d'en conclure la densité *que devait avoir* la vapeur du carbone, laquelle se trouvait être un élément commun à toute la série des produits qu'il avait à étudier. Il l'inféra de la composition de l'acide carbonique, en supposant que 1 volume de ce gaz renferme 1 volume d'oxygène, plus 1 volume de vapeur de carbone, sans condensation; et le nombre ainsi obtenu lui servit ensuite avec succès, pour exprimer tous ses autres produits par des rapports simples de volumes, d'où résultait leur composition pondérale. Evidemment, la certitude de ce genre d'induction n'est pas absolue, puisqu'elle se fonde sur le rapport de contraction ou d'expansion que l'on attribue aux vapeurs composantes, dans les vapeurs composées, en leur appliquant de plus la loi de Mariotte, qui ne s'y adapte pas avec une entière rigueur. Mais, sauf ce dernier inconvénient, qui est inévitable, le rapport supposé devient d'autant plus probable, qu'on l'établit, dans chaque cas, sur des analogies de combinaisons plus intimes. D'ailleurs, d'après le principe général de la loi, si le nombre représentatif de la densité auquel on est conduit n'est pas le véritable, il en sera toujours, approximativement, un multiple simple; ce qui permettra de l'introduire dans la série des combinaisons, sans dénaturer leurs relations

* Annales de Chimie, tome xcv. page 136, et suiv.

essentielles. Cette extension donnée par Gay-Lussac à la théorie des proportions définies, a été une des innovations les plus hardies et les plus fécondes que l'on ait apportées, de nos jours, dans la science chimique.

Poursuivant toujours la même vue, il montra peu après, dans une courte note, comment des corps composés, physiquement très divers, étant considérés à l'état de gaz, peuvent être idéalement constitués par des grouppes de vapeurs, représentant d'autres corps, toujours les mêmes, mais assemblés en nombres divers et simples, de volumes gazeux.* Cette conception est reconnue aujourd'hui comme la seule rationnelle et générale, par laquelle on puisse exprimer, et mettre en évidence, les rapports de composition des substances organiques entr'elles. Il ne faut pas imputer à ce principe l'abus qu'on en a pu faire, en prenant, contre l'intention de son auteur, ces possibilités de représentation, pour des réalités absolues, comme cela est arrivé trop souvent.

L'espace nous manque, pour analyser, même pour mentionner, une foule d'autres travaux importans de Gay-Lussac. Nous avons pu citer seulement, parmi leur grand nombre, ceux qui nous ont paru le mieux le caractériser. Pendant les années qu'il y consacra, son talent reconnu l'éleva, sans effort, à tous les honneurs des sciences. Professeur de physique, ou de chimie, dans plusieurs établissemens publics, il porta dans son enseignement, comme partout ailleurs, la dignité simple, et un peu froide de ses manières, avec la netteté, la droiture, la justesse d'appréciation, qui étaient habituelles à son esprit. Mais ensuite, une autre carrière, sinon plus belle, ou plus attrayante, du moins plus profitable à ses intérêts, de fortune, s'ouvrit pour lui, et l'absorba bientôt presque entièrement. Depuis 1805, il était membre du comité consultatif des arts et manufactures, établi près le ministère du commerce. En 1818, on l'attacha aussi à l'administration des poudres et salpêtres. Il s'était marié en 1808, à une personne dont l'affection répondait à la sienne, et il était devenu père de famille. Dans ces circonstances, il parut regarder désormais comme un devoir de tourner son talent vers les applications. Ce fut ainsi qu'il publia successivement des instructions pratiques d'une grande utilité, sur la fabrication de l'acide sulfurique hydraté, sur les essais des chlorures décolorans, des alcools, des alcalis employés aux usages du commerce, &c. On y retrouve son même caractère, d'adresse ingénieuse, d'exactitude, d'habileté prudente, adapté avec une rare intelligence à la simplicité des manipulations industrielles. En cherchant à se rendre l'industrie profitable, il voulait aussi l'avancer; et son intégrité n'aurait consenti, pour aucun prix, comme le font tant d'autres, à propager, ou à étayer par l'autorité de son nom, des procédés, ou des entreprises, dont le succès ne lui aurait pas paru assuré scientifiquement. C'était toujours le même homme, dans une autre sphère. En 1829 il fut nommé essayeur du bureau de garantie de la monnaie, emploi très lucratif; et, au lieu des procédés de la coupellation employés exclusivement jusqu'alors, il imagina, et introduisit dans les opérations qu'on lui confiait, l'essai de l'argent par la voie humide, ce qui leur donna un dégré nouveau et remarquable, de facilité, de rapidité, de

* Annales de Chimie, tome xcv. page 311.

précision. Il prit aussi de sérieux intérêts dans une fabrique de glaces, qui fûrent suivis de grands avantages réciproques. Depuis qu'il fut entré dans cette voie des affaires, il dut, pour sa consistance même, désirer d'avoir une place dans les grandes assemblées politiques. Il fut nommé membre de la chambre des députés en 1831 ; puis, en 1839, membre de la chambre des pairs. Mais, heureusement pour lui, il échappa aux inconvéniens de ces positions périlleuses, parceque, n'y remplissant que le rôle passif d'un savant considéré, il s'arrangeait politiquement à peu près de tout, et ne faisait obstacle à personne. Cette dernière phase de sa vie fut donc honorablement industrielle et sociale, plutôt que scientifique. Il est mort le 9 Mai 1850, d'une atrophie du cœur, dans sa 72e année, après s'être longtems bercé de l'espérance de revenir un jour aux nobles travaux qui avaient fait sa célébrité.

The Rev. William Kirby, M.A., Rector of Barham, Suffolk, was born at Witnesham Hall in that county, the residence of his father, who was by profession a solicitor, Sept. 18, 1759. His mother was Lucy, daughter of Mr. Daniel Meadows of the same parish. His grandfather, John Kirby (born 1690), as we learn from a notice by a near relative in the 'Literary Gazette', wrote 'The Suffolk Traveller,' a work of considerable reputation in its day; and his uncle, Joshua Kirby, was the author of 'Dr. Brook Taylor's Perspective made easy.' This Joshua Kirby was the intimate friend of Gainsborough, who directed by his will that he might be buried by his side—a desire which was carried into effect,—and was appointed to the office of Comptroller of the works at Kew, by His Majesty George III., with whom he was a great favourite. Mrs. Trimmer was his daughter, and consequently first cousin to Mr. Kirby.

Mr. Kirby was educated at the Grammar School at Ipswich, whence he removed in his seventeenth year to Caius College, Cambridge. Here he pursued his studies with diligence, and laid so good a foundation, that he subsequently earned the reputation of being a sound and accurate scholar. In the year 1781 he took the degree of B.A.; in the year 1782 he was admitted into holy orders, having been nominated by the Rev. Nicholas Bacon to the joint curacies of Barham and Coddenham. By his exemplary conduct in the discharge of his parochial duties, he so gained the esteem of Mr. Bacon, that he left him by his will the next presentation to the rectory of Barham; to this he was inducted in the year 1796, so that for sixty-eight years he exercised his ministry in the same charge, residing also in the same parsonage-house.

Mr. Kirby's first love for natural history was awakened by his mother occasionally lending him, when a boy, some of the more showy foreign shells from her cabinet to play with, as a reward for good conduct. He was accustomed to ask for his favourite Shells by their names, which he learned from his mother, and to this early pleasurable association of the nomenclature of objects of natural history, with the objects themselves, it is probable that he

derived no small portion of the pleasure which he felt in his maturer years, from the dry and tedious investigations (as some would have deemed them) in which he delighted to engage, to ascertain the names and the relative priority of those given by former naturalists, to any plant or insect before him.

Mr. Kirby's taste for natural history lay dormant during his university studies, as so often happens in similar cases, but was re-excited as soon as he entered on his curacy, proving how important it is that an early bias in this direction should be given in youth, and how vast a fund of enjoyment is lost to our clergy from the unfortunate neglect of this science in our schools. The land-shells about Barham, which it is probable from his boyish predilections he would first collect, being soon exhausted, he naturally turned his attention to botany, and in the course of time became thoroughly acquainted with the flowering-plants around him, which he investigated by the aid of Smith's 'English Botany' and other botanical works. When these were becoming scarce, that he began to observe the Cryptogamic tribes, may be inferred from his having one day in the spring of 1809 conducted Sir W. Hooker and Mr. Spence to the habitat of a rare species of Marchantia? on a particular bank, several miles from Barham,—the excursion ending in a ludicrous adventure, which in after years he often related when speaking of his natural history rambles.

How far his Cryptogamic investigations would have been pursued, it is impossible to say; but it is not unlikely that but for an incident which turned his thoughts into another channel, that of Entomology, he might have been led to carry them out with the characteristic ardour with which he followed up every pursuit in which he engaged, and that Mosses, Lichens and Confervæ would have absorbed that attention which he afterwards gave exclusively to insects.

The incident referred to, which he has himself very graphically related in the 'Introduction to Entomology' (vol. ii. p. 227), was his accidentally observing and being led to admire and preserve a yellow Cow-lady (*Coccinella 22-punctata*), which was crawling on his window-sill. So little up to this time had he attended to insects, that this species, which is not uncommon, struck him with surprise and admiration, and led him forthwith to search for others; and every new acquisition increasing his wonder and delight with the vast and beauteous region of Nature thus opened to his view, he threw aside botany and devoted himself wholly to entomology—thus affording another proof, how great events spring from small causes.

While collecting and studying the insects round Barham for some years, during which he communicated several valuable papers to the Linnean Society, of which he had become a Fellow, he particularly directed his attention to the tribe of wild-bees, and finding how few of them were described and how little they had been systematically studied, he made notes on the various species and families, till at length his materials were so considerable as to become

the foundation of his great work, the 'Monographia Apum Angliæ,' which appeared in 1802, in two volumes 8vo. This work would have been thought remarkable as the unaided production of a country clergyman and entomologist of no long standing, had it been a mere description of a greatly extended number of the previously recognised British species of the Linnean genus *Apis*; but when we consider that he had not only brought together and described upwards of two hundred species of the tribe, but with an admirable largeness and correctness of view had divided them into numerous families and subfamilies, so natural, that most of them have since been adopted as genera, and that he took lessons in the art of etching for the express purpose of being enabled to give a correct idea of the parts of the mouth on which his divisions were mainly founded, we shall not be surprised that it excited the warmest admiration of British and foreign entomologists on its appearance, and at once elevated him to the rank of one of the first entomologists of the age.

During the next five or six years, Mr. Kirby collected materials for, and contributed to the Linnean Society, two very valuable papers on the genus *Apion*, in which he described a great number of new species.

In the year 1808, Mr. Spence, who had carried on an active entomological correspondence with Mr. Kirby for the preceding two or three years, proposed to him, with the view of remedying the want of a good English introduction to the science, that they should jointly write one—a proposal to which Mr. Kirby at once assented, as he did to Mr. Spence's subsequent extension of it, that the work should not be merely technical as was his first idea, but be thrown into a popular form, comprising under various heads all the known facts relative to the habits and economy of insects, and their noxious or useful properties, &c., and in the shape of letters so as to admit of a more discursive mode of treating the subject. The general plan having been thus agreed on, Mr. Spence spent several weeks with Mr. Kirby at Barham, in the spring of 1809, to fill up its details, and to commence the letters on external anatomy and orismology, which were the portions first written, and on this occasion and on similar visits in subsequent years, every term and its definition were discussed by the authors, and the other letters written by each, jointly criticised.

It would not be easy to overrate the influence which this work (of which vol. i. was published in 1815, vol. ii. in 1817, and vols. iii. and iv. in 1826) had on Mr. Kirby's subsequent entomological career. Not only did it supply, under its various heads, suitable places for introducing from his note-book, his numerous detached observations during many years, on the manners, economy, &c. of insects, which but for this opportunity would probably have been lost to the world, but the necessity of extending his former studies as to the anatomy and nomenclature of the parts of bees, to those of all the other orders, and of investigating many new points of their history, led to a great accession to his previous knowledge, which

bore ample fruit both in the work in question and in his future ones.

Of these, following his admirable paper on his new order "*Strepsiptera*" and his "Century of Insects" in the 'Linnean Transactions,' the most important was his 'Bridgewater Treatise,' "On the History, Habits and Instincts of Animals," published in the 76th year of his age, in which he brought together a vast number of curious and important facts in the natural history of animals, and urged their application in proof of the wisdom and goodness of the Creator with that persuasive and pious ardour with which he always endeavoured to lead his readers, like himself, "to see God in all things."

This is not the place to enumerate the titles of his numerous less important papers in the 'Zoological Journal,' and other periodical works on natural history, and it must suffice to notice his last great work, the entomological portion of the 'Fauna Boreali-Americana' of Sir John Richardson, published in a quarto volume in 1837, in which he described the insects of the northernmost region of America with a largeness of grasp as to their generic and family relations, and a clearness and accuracy as to their specific characters, fully proving that at the very advanced age of nearly eighty, his entomological zeal and acumen had not diminished.

Some time before the publication of this work, Mr. Kirby, feeling that his entomological career was appropriately closed by it, gave his whole collection of British and Foreign Insects to the Entomological Society of London, of which he had been elected the Honorary President on its establishment in 1833—a noble gift, for which future entomologists, who will thus have access to the very species he described in his various works, will feel a deep debt of gratitude.

Constant and unwearied as were Mr. Kirby's entomological labours, they never encroached on his professional and social duties. His parishioners of every class looked up to him as a father, on whose advice, sympathy and assistance they might confidently rely, and the whole tenor of his long life proved him to be at once one of the kindest of friends and most simple-minded, warm-hearted and pious of men.

Mr. Kirby was elected a Fellow of the Royal Society in 1818, and became one of the Linnean Society soon after its institution in 1788. Besides being Honorary President of the Entomological Society and President of the Ipswich Museum, he was a Fellow of the Geological and Zoological Societies, and an Honorary Member of several foreign societies. He died July 4, 1850, aged ninety years and ten months, and was buried in the chancel of his church at Barham.

Sir Robert Peel was born on the 5th of February, 1788, at Bury, in Lancashire. His father was one of our greatest manufacturers, a very wealthy and a very able man, remarkable for the vigour of his mind and the extent of his practical knowledge.

Sir Robert was educated at home with great care till of a proper

age for a public school, and then was sent to Harrow, where, applying himself with unusual diligence, he made rapid progress in his studies, and in his seventeenth year entered Christchurch, Oxford.

The new examination statutes had been recently passed, subjecting all persons who proposed to take a degree to a public examination, with a graduated scale of honours proportioned to their proficiency, either in a mixed course of Classics and Aristotelian Philosophy, or a course of Mathematics and Physics, the first class being the highest honour. Sir Robert Peel at once undertook to read for the highest honours in both courses, and was the first person who succeeded. In 1808 he graduated, and after that he does not appear to have pursued his scientific studies further, but entering the House of Commons as representative of the Borough of Cashel, he devoted himself exclusively to politics. He was soon appointed Under Secretary for the Home Department, and filling in succession the various offices, the highest prizes of a successful political career, he eventually became Prime Minister.

With science politicians in this country have not usually much to do: Sir Robert Peel, however, was a Member of the Committee in which the scientific pensions originated, and on several occasions as a Minister of the Crown he awarded scientific pensions. Fully alive to the importance of science as the basis of the engineering and manufacturing pre-eminence of this country, he was in the constant habit of consulting scientific men whenever he had any new commercial measures to bring forward, or whenever a difficulty arose which called for scientific assistance; but he does not seem to have devoted any portion of his leisure moments to scientific reading, or to have kept up the mathematical knowledge he had early acquired at Oxford; and he appears to have regarded science with interest rather for the sake of its applications to practical purposes than as a high and ennobling pursuit.

Looking to Sir R. Peel's great abilities, his commanding influence, his unrivalled powers as a man of business, his success at Oxford, which was evidence that he had then the power of using the keys of exact knowledge, it is natural that scientific men should have been anxious for his assistance in the management of the Associations for the Advancement of Science. He was a Trustee of the British Museum, and was most efficient; but when it was intimated to him that there was a general wish that he should permit himself to be put forward more prominently either as a President of the British Association, or in some other way, he declined, stating that he was not qualified, as science was not one of his pursuits. On the 29th of June, having been in the House of Commons till four o'clock in the morning, he attended an early meeting of the Royal Commissioners for the Exhibition of 1851, assisting in the transaction of business with his usual vigour and judgement, and soon after the meeting he was thrown from his horse and received the fatal injuries of which he died on the 2nd of July.

On the motion of the Marquis of Northampton, seconded by Sir Harry Inglis, Bart., the best thanks of the Society were tendered to the President for his excellent Address, and his Lordship was requested to permit the same to be printed and circulated to the Society.

The Statutes relating to the election of Officers and Council having been read, and Mr. Spence and Mr. Yates having, with the consent of the Society, been nominated Scrutators, the votes of the Fellows present were collected.

The following Noblemen and Gentlemen were reported duly elected Officers and Council for the ensuing year:—

President.—The Earl of Rosse, K.P., M.A.

Treasurer.—Lieut.-Col. Edward Sabine, R.A.

Secretaries. { Samuel Hunter Christie, Esq., M.A.
Thomas Bell, Esq.

Foreign Secretary.—Captain W. H. Smyth, R.N.

Other Members of the Council.—John Joseph Bennett, Esq.; William Bowman, Esq.; Sir Benjamin Collins Brodie, Bart.; The Rev. Professor Challis, M.A.; Lieut.-General Sir H. Douglas, Bart., G.C.B.; Sir P. de Malpas Grey Egerton, Bart.; John Forbes, M.D.; Marshall Hall, M.D.; Gideon A. Mantell, Esq., LL.D.; Professor W. Hallows Miller, M.A.; Sir R. Impey Murchison, M.A.; Richard Phillips, Esq.; Rt. Hon. Sir Frederick Pollock, M.A.; George Rennie, Esq.; Edward Solly, Esq.; Lord Wrottesley.

The thanks of the Society were given to the Scrutators for their trouble in examining the lists.

The President appointed the following gentlemen Vice-Presidents of the Society:—Col. Sabine, R.A., Sir Benjamin Collins Brodie, Bart., Sir P. de Malpas Grey Egerton, Bart., Sir R. I. Murchison, Right Hon. Sir Frederick Pollock, and George Rennie, Esq.

The following is a statement of the Receipts and Expenditure during the past year:—

Statement of the Receipts and Payments of the Royal Society between Nov. 30, 1849, and Dec. 1, 1850.

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance in the hands of the Treasurer at the last Audit ..				553	4	9
Weekly Contributions, at one shilling				44	4	0
Quarterly Contributions at £4				1102	0	0
17 Admission Fees				170	0	0
2 Compositions for Annual Payments at £60				120	0	0
4 Compositions for Annual Payments at £40				160	0	0
One year's rent of estate at Mablethorpe: due at Michaelmas 1849	125	0	0			
One year's Income Tax	3	13	0			
				121	7	0
One year's rent of estate at Acton: due at Michaelmas 1850	70	0	0			
One year's Income Tax	2	0	10			
				67	19	2
One year's Fee farm rent of lands in Sussex: due at Michaelmas 1850				19	4	0
One year's rent from Royal College of Physicians				3	0	0
Dividends on Stock:—						
One year's dividend on £14,000 Reduced 3 per cent. Annuities	420	0	0			
Less Income Tax	12	5	0			
				407	15	0
One year's dividend on £6385 3*s.* 8*d.* 3 per cent. Consols	191	6	6			
Less Income Tax	5	7	2			
				185	19	4
Half a year's dividend on £600 3 per cent. Consols	9	0	0			
Less Income Tax	0	5	3			
				8	14	9
One year and a half's dividend on £3452 1*s.* 1*d.* 3 per cent. Consols, produce of sale of premises in Coleman Street	155	6	9			
Less Income Tax	4	10	6			
				150	16	3
Carried forward..........				3114	4	3

	£	s.	d.	£	s.	d.
Brought forward..........				3114	4	3
Donation Fund.						
One year's dividend on £5331 10*s.* 8*d.* Consols	159	18	6			
Less Income Tax	4	13	0			
				155	5	6
Rumford Fund.						
One year's dividend on £2430 12*s.* 5*d.* Consols	72	17	9			
Less Income Tax	2	1	9			
				70	16	0
Fairchild Fund.						
One year's dividend on £100 New South Sea Annuities				3	0	0
Bakerian Lecture and Copley Medal Fund.						
One year's dividend on £366 16*s.* 1*d.* New South Sea Annuities	10	18	0			
Less Income Tax	0	6	2			
				10	11	10
Wintringham Fund.						
One year's dividend on £1200 Consols	36	0	0			
Less Income Tax	1	1	0			
				34	19	0
Miscellaneous Receipts:—						
Sale of Philosophical Transactions, Abstracts of Papers, and Catalogues of the Royal Society's Library				298	9	3
Mablethorpe Tithe Suit Release....................				42	18	5
Total Receipts......				£3730	4	3

PAYMENTS.

	£	s.	d.	£	s.	d.
Fairchild Lecture.—The Rev. J. J. Ellis, for delivering the Fairchild Lecture for 1850				3	0	0
Bakerian Lecture.—Thomas Graham, Esq., for the Bakerian Lecture for 1850				4	0	0
Books purchased:						
Dulau and Co.: for Books	43	3	2			
Taylor: for ditto	29	16	2			
Nutt: for ditto.......................	5	15	9			
Gould: for ditto	12	12	0			
Curtis: for ditto	21	0	0			
Second-hand ditto	16	2	6			
				128	9	7
Carried forward.........				135	9	7

				£	s.	d.
Brought forward..........				135	9	7
Salaries :—						
S. H. Christie, Esq., one year, as Secretary..	105	0	0			
Thomas Bell, Esq., one year, as Secretary ..	105	0	0			
Ditto for Index to Phil. Trans.	5	5	0			
Col. Sabine, one year, as Foreign Secretary..	20	0	0			
Charles R. Weld, Esq., one year, as Assistant-Secretary	300	0	0			
Mr. White, one year, as Attendant.........	100	0	0			
G. Holtzer, one year, as Porter	30	0	0			
Ditto, for extra Porterage	10	0	0			
				675	5	0
Purchase of £600 0*s*. 0*d*. 3 per cent. Consols				573	0	0
Fire Insurance, on the Society's Property				45	1	6
Gratuity to Bank Clerks				1	1	0
Bills :—						
Taylors :						
Printing the Phil. Trans., 1849, part 2 ..	217	13	3			
Ditto, 1850, part 1....................	168	13	3			
Ditto, Proceedings, Nos. 73—75; Circulars, Lists of Fellows, Ballot-lists, Statement of Payments, and Minutes of Council; Government Grant Committee, Notices, &c. &c.	135	18	0			
				522	4	6
Basire :						
Printing Plates in Transactions, 1849, part 2	95	12	4			
Engraving, 1850, part 1..............	98	18	0			
Ditto, part 2	86	4	0			
				280	14	4
Dinkel :						
For Lithography	8	8	0			
Wing :						
For ditto	16	0	0			
Scharf :						
For ditto	15	0	0			
F. Gyde :						
For Woodcutting	17	1	6			
Walker :						
For Printing Charts	104	12	0			
				161	1	6
Bowles and Gardiner :						
Paper for the Phil. Trans., 1849, part 2, and 1850, part 1				188	2	0
Carried forward..........				2581	19	5

				£	s.	d.
Brought forward..........				2581	19	5
Gyde:						
Boarding and Sewing 800 Parts of Phil. Trans., 1849, part 2	11	4	0			
Ditto, 1850, part 1...................	11	4	0			
Ditto, Extra binding	25	19	11			
				48	7	11
Tuckett:						
Bookbinding	40	5	0			
Limbird:						
For Stationery	19	17	11			
Saunderson:						
For Shipping Expenses	11	19	1			
Norman:						
For ditto	3	16	3			
Brecknell and Turner:						
Candles, and Lamp Oil	35	4	6			
Arnold:						
For Coals	30	7	0			
Meredith:						
Mats, Brushes, Fire-wood, &c.	8	7	6			
Cubitt:						
For repairs and relaying Carpets, &c.......	22	13	0			
Slack:						
For Repairs	2	7	11			
Shoolbred:						
For Carpets, Curtains and Matting........	61	12	11			
Woodward:						
For Cases and Shelves	3	19	6			
Sharpus:						
For China	3	7	4			
Humphries:						
For Livery	5	10	0			
Tea, Waiters, &c. at Ordinary Meetings	33	9	6			
Higgins:						
Valuing Estate at Mablethorpe	5	5	0			
Ditto, draining ditto	100	0	0			
Seguier:						
For restoring two Portraits	8	8	0			
Coombe:						
For Picture Frames	4	0	0			
				400	10	5
Taxes:						
Land and Assessed Taxes	21	5	0			
Income Tax	4	19	2			
				26	4	2
Carried forward..........				3057	1	11

				£	s.	d.
Brought forward..........				3057	1	11
Rumford Fund:						
Mr. Wyon, for Medals	64	0	0			
M. Arago, Dividend	77	12	0			
				141	12	0
Donation Fund:						
Dr. Hofmann, for Chemical Investigations	100	0	0			
Mr. Miller, for Meteorological Observations	50	0	0			
Mr. Newport, for Physiological Investigations	50	0	0			
Dr. Frankland, for Chemical Investigations	50	0	0			
				250	0	0
Petty Charges:						
Postage and Carriage..................	43	4	2			
Expenses on Foreign Packets, &c.........	12	6	11			
Stamps	2	5	0			
Charwoman's Wages..................	27	16	6			
Extra Cleaning	4	12	0			
Miscellaneous expenses	34	7	1			
				124	11	8
Balance in the hands of the Treasurer				156	18	8
Total....				£3730	4	3

GEORGE RENNIE, *Treasurer.*

November 30*th*, 1850.

Estates and Property of the Royal Society.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.). Rent £110 per annum.

Estate at Acton, Middlesex (33 acres). Rent £70 per annum.

Fee farm rent in Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

£14,000 Reduced 3 per cent. Annuities.

£19,399 7*s*. 10*d*. Consolidated Bank Annuities.

£366 16*s*. 1*d*. New South Sea Annuities.

The Receipts during the past year, exclusive of the Balance, amounted to:—£3176 19*s*. 6*d*.

The Expenditure during the same period, exclusive of the sum of £600 0*s*. 0*d*. invested in the Funds, was:—£3000 5 7

The Balance in hand, now belonging to the Donation Fund, is £115 11*s*. 2*d*.

Annual Contributions.

1830	£363	4	0
1831	286	0	0
1832	255	6	0
1833	283	7	6
1834	318	18	6
1835	346	12	6
1836	495	0	0
1837	531	0	0
1838	599	4	0
1839	666	16	0
1840	767	4	0
1841	815	12	0
1842	910	8	0
1843	933	16	0
1844	1025	16	0
1845	1010	0	0
1846	1074	0	0
1847	1116	8	0
1848	1122	16	0
1849	1130	16	0
1850	1146	4	0

In the notice of admissions, those of the Right Hon. Lord John Russell, M.P. and the Most Reverend The Lord Archbishop of Canterbury, at the ordinary Meeting of the Society, on the 25th January 1849, were omitted.

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END OF THE FIFTH VOLUME.

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